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GENTLEMEN OF THE PSYCHOLOGICAL ASSOCIATION:

The time and manner of the organization of this Association seem to me significant of certain important truths which concern the science in whose behalf the organization has been effected. Without undue modesty we should perhaps speak of ourselves as the youngest—the most nearly embryonic—of all similar scientific bodies; and it is, of course, well known that many workmen in other lines of scientific endeavor, and even some of the most notable and helpful among ourselves, still deny that psychology is entitled to be called a 'science.' On the other hand, it is not unbecoming pride which leads us to maintain that no similar organization is more hopeful, more disposed to be creditably aggressive, than are we. For few, if any, of the most firmly established and highly accredited scientific associations can rely upon a more devoted and well-trained membership, or upon more interest—both popular and permanent—in the results of their researches and speculations, than can those formed for the cultivation, in the use of modern methods, of the science of psychology.

Such a position as that which we occupy has certain disadvantages and certain equally great advantages. It cannot, indeed, be truthfully claimed that psychology has at present

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the same settled and accepted principles of method as those which belong—for example—to the modern sciences of physics and chemistry. Possibly—though doubt is certainly permissible here—in respect of its possession of an accredited method, it is not even the peer of biology, or of a so-called 'social science.' Neither is it possible for psychology, at least as yet, to formulate its ascertained facts, and announce the discovery of universal 'laws,' with the precision which belongs to the more advanced physical sciences. On the other hand, I am bold enough (perhaps *rash* enough would seem the more appropriate word) to predict that some of the most widely accepted of these physical formulas are destined to be thoroughly shaken up, in the not far away future. But, however this may be, there is always a certain advantage in the plasticity, the superior mouldableness, of the origin of scientific products and their developments. And if psychology, as a science, must be considered embryonic in its present stage, there is on this account the more opportunity for a band of students and investigators, such as we aim to be, to contribute something important to its more stable and higher evolution.

Now it seems to me that the large and final success of an Association like this will depend very conspicuously upon the attitude which its members maintain toward the three following classes of inquiries. I say 'classes' of inquiries, because each of the questions which I am about to raise includes an indefinite number of subordinate questions. As to these subordinate questions, probably no two members of this Association could be found in perfect agreement. But as to the right general attitude toward each of the three classes of inquiries, it seems to me possible that we may start our special lines of work with something approaching a common consent. A detailed discussion of even such general questions, and a defence of the attitude which I think should be taken toward them, would be quite too much of a task for the present occasion. I shall limit myself to a brief statement, followed by some rather indefinite remarks upon what seems to me to be the right attitude toward each. I shall be content with gaining something in comprehensive-

ness of view, and in largeness and freedom of spirit, even if I lose much as respects precision and satisfactoriness of proof.

In other words, gentlemen, let us allow our eyes to wander with an æsthetical and ethical, rather than purely scientific, intent over our broad and fair domain, before we settle down, as a well-organized colony, to its minuter exploration and cultivation.

The three classes of inquiries to which reference was just made are the following: (1) What is the relation in which the statistical and experimental investigation of mental phenom ena stands to the total science of psychology, in the larger meaning of the latter words? (2) What is the relation in which the science of psychology, thus understood, stands to that interpretation of the external world and of human life, in its yet larger and profounder experiences, which we are wont to call philosophy? (3) What is the relation in which the science of psychology stands to conduct and to the practical welfare of mankind? More briefly expressed: How shall we regard the science of mental life as related to the methods and conclusions of the most nearly allied physical sciences, to philosophy, and to human action and character?

Now, if I were to speak my mind at all fully in answer to either of these three questions, I should doubtless find the amount of assent which my words commanded varying for each of the three. As to the last of the three questions, it would probably be possible to receive the adherence of all my auditors; as to the second of the three (namely, the relation of psychology to philosophy), a pretty general agreement might perhaps be reached; but as to the nature of the science called psychology, and as to the use of laboratory and other allied methods for its cultivation, perhaps any one of our number, if addressing the Association, should be amply satisfied if he had succeeded in carrying the assent of a bare majority. This last remark is made in passing, partly as a matter of rhetorical policy; for the order of treatment which I have adopted compels me to speak of the most controverted subjects first.

First, then, as to the question of method, and of the possibility of rendering psychology more truly scientific, by use

of right method; as well as of the value and limits of the more modern statistical and experimental researches. On all these matters I begin with an exhortation addressed not less to myself than to all my colleagues in the common work. Let us all always be just: nay, let us be something more than merely just; let us be generous. And let our generosity include all workmen of all times, with their works, from Aristotle's *De Anima* to the latest thesis by the youngest aspirant for the degree of Doctor of Philosophy,—even if months of painstaking experiment in some German or American laboratory have led him to merely 'negative' results. Let this same generosity also include all methods of dealing with mental phenomena; from experimenting with key and chronometer, through thousands of trials in reaction-time and elaborate mathematical discussion of general averages, to the introspective seizure of some rare happening in individual consciousness, with the felicitous guess which genius makes as to the meaning of the fact thus surprised; or to the reflective study of that artistic delineation of soul-life in which the best novels, poems, and dramas are so wonderfully successful.

Nor can I approve of the proposal to restrict the use of the words 'science' and 'scientific'; or of the denial that psychology *is* a science, or of the refusal to accept as *scientific* other contributions than those of the physiological or psychophysical laboratory. Apropos of the correct and courteous use of these terms, I recall a colleague of my younger days, a young professor of physics; this ardent 'scientist' boldly denied the right of any other branch of human knowledge to the term 'science'; with him, there was only one *science*,—namely, physics. But, on the other hand, I shall not soon forget the reply, made by one of the most distinguished investigators and writers on this subject, to my question (half jestingly put), whether he considered meteorology a science. Said the veteran: "Meteorology is *just as much* of a science as geology is."

Now no fixed line can ever be drawn between science and ordinary knowledge; and science begins whenever and wherever facts begin to be carefully observed and classified, and attempts at explanation, by way of stating the customary

forms of the occurrence of the facts in relation, are made. To affirm that psychology is not a science, whether reference be had to the study of mental phenomena by the so-called introspective or by the so-called experimental method, seems to me both philologically and historically indefensible. While to postpone the gift of the title until some law, like that of gravitation in physics or of chemical æquivalency in chemistry, has been discovered, is to assume, unwarrantably, that some *such* law is actually followed by the phenomena. Such an assumption is, itself, at least premature and unscientific; even if we are not justified in saying that the very nature of psychical facts, and of their origin and sequence, is such as to render it forever unrealizable. In brief, there is every reason why we should be both just and generous in our use of terminology. This Association is formed for the advancement of a *science* already existing, and, indeed, like all the other principal sciences, some centuries old. We of to-day have entered into the inheritance of past ages; and it is becoming for us to do so with generous acknowledgment of what the past has done for us. And yet, although we are children of the ages, we are pre-eminently children of the present age. For a certain way of studying the phenomena of mental life is comparatively modern; and the hopes which are entertained respecting results from this method are by no means altogether misleading.

This last remark introduces certain considerations respecting the relation of introspection and the use of statistics and experimentation in psychology. I need not speak in detail of the burning and strife which have too often accompanied the mere mention—not to say, the discussion—of this subject. I venture to hope that I speak for the great majority of this Association when I say that this feeling is to be deprecated; and, except so far as all controversy, however conducted, helps in a measure to elicit truth at last, it is to be distinctly avoided. Whoever takes a wide historical and philosophical view of the evolution of science in general—I do not say simply of psychology, in particular—can sympathize fully with neither of the two extreme views. He will neither, on the one hand, quake with fear lest the foundations of the

world's stock of truths in ethics and philosophy are to be undermined by the discovery of the function of Broca's convolution, or of the laws of reaction-time where apparent choice is concerned; nor, on the other hand, will he undertake to deny the verity of æsthetical, ethical, and religious consciousness, or pride himself on his ability to dispense with introspective psychology and philosophy, because of some new device in mechanism to aid the solution of certain subordinate psycho-physical problems.

The question how far laboratory and other methods akin to those of the most advanced physical sciences can be used in the development of a scientific psychology will answer itself only in the course of history. It is always a venturesome thing to lay down limits that anticipate the requisite experience. That can be which will be; and what will be cannot always be precisely predicted by means of what now is. Yet certain observations occur to me which seem more or less certain of realization. That no method can be developed in psychology which will enable us to dispense with introspection, or which will cease to be very largely dependent, for its own value, upon the value of the introspection which accompanies it, is too obvious to require discussion. Of course, the proposal wholly to get rid of self-consciousness as the medium of knowledge of the phenomena of consciousness is absurd. And however we may seem compelled to interpret the language of any advocate of experimentation and 'objective' observation *in the stead of* introspection, we can scarcely believe that his proposal is to be seriously and intelligently understood. The results of any 'series' of experiments, the generalizations from any 'pile' of statistics, become material for psychology only when they are interpreted in terms of consciousness. For scientific psychology *is* the science of the phenomena of consciousness, *as such*. And no interpretation of consciousness is possible in any terms whatever without self-consciousness. Every intelligent and sincere worker by laboratory methods knows that there is nothing of more doubtful scientific value than are the results obtained when the man behind the key is reacting in the interests, as it were, of his own self-consciously or unconsciously adopted theory;

unless, indeed, it be the interpretation of results obtained from an unprejudiced reacting agent by some prejudiced theorizer. Twist the matter as we may, we cannot get rid of the fact: skill in introspective observation and analysis sits at one end of the series of experiments as witness being examined, and at the other end as judge pronouncing after, or even before, the examination of the witness. It is plainly worth while to remark in passing that the same thing is true, though in far less degree, in all the physical sciences. The history of biology, of geology, and even of astronomy is full of examples of failure to arrive at truth objective and universal through lack of skill in self-knowledge. Hence the safe conclusion that a scientific psychology is the handmaid of all the sciences.

Furthermore, any attempt to separate introspection from experimentation and the more objective estimate of statistical material is as impolitic as it is plainly impossible. In past time the science of psychology has been advanced far more by those guesses at the truth based upon *my* truth,—those leaps from what is self-consciously discerned as *in me* to what belongs to all men, to human nature as such,—which characterize the "born psychologist," than by long series of trial experiments or by vast collections of "data" so called. Nor am I sure that this will not always continue to be so. Here again, however, the method of psychology is not so wholly unlike that by which the physical sciences have grown. They, too, have made their great advances chiefly through the intuitive flashes of that genius which sees the general and the universal as it manifests itself in the particular. In psychology, as in these physical sciences, the truth which Aristotle recognized, of course, always holds: there can be no *science* of that which is individual merely. But in psychology more, by far, than in the physical sciences, the observation and skilled interpretation of the facts of individual experience are likely to lead directly to what is true and valuable for the entire species.

Once more, it seems to me that there are certain factors and aspects of all, even the commonest mental life, which will never readily lend themselves to refined methods of experimental analysis and interpretation; which will never yield to

the attempts, however persistent, of the collectors of 'data.' I am well aware that my opinion here will by no means command universal assent. It will probably seem to some that I am violating my own caution, not to limit in lofty *a priori* fashion the possibilities of triumph which lie before the new methods of solving psychological problems.

In illustration of my meaning, however, let me call attention to the following facts. We have had of late many considerable volumes on psychology; as, indeed, voluminous works abound on all the modern sciences. But perhaps we do not often enough consider how exceedingly meagre, as compared with the wealth and complexity of actual mental life, are the most voluminous of these treatises. Let the plain man read carefully through the biggest of all these books; and the astonishing thing is that so large a part of his daily experience is, not simply left unexplained to his satisfaction; it is not even treated at all. This is, of course, no sure proof that psychology is still in a lamentably backward condition. It is an illustration of the general truth that all human science is but patches of a shallow, superficial stratum, dimly lit through occasional rifts in the clouds, over the fathomless depths of the ocean of reality. For example, how absolutely dumb is all our most advanced evolutionary biology, when the common gardener asks for an explanation of the changes through which pass the phylloxera that are destroying the roots of his vines, or the moths that feed upon the leaves of his fruit-trees!

In further illustration of my meaning let me—though with a protest looking toward its definitive and final rejection—adopt for the moment the customary division of mental phenomena into knowledge, feeling, and will. It is simple matter of fact that, thus far in the development of laboratory and statistical methods for dealing with mental phenomena, it is the ever-present sensation-content of all these aspects of mental life which has chiefly, and almost exclusively, been the subject of treatment. This is, in part, perhaps the reason why some who are most nobly impatient of the limitations which have hitherto surrounded the use of experimental methods are so strongly inclined to identify feeling with quality of sensation, 'pleasure-

pain'-wise, and volition with dominant stress of sensation. But let us place the plain man in the presence of any common thing, and let him attain what he calls a 'knowledge' of that thing, and then summon the psychologist who is most expert in laboratory methods, and most learned in the results of such methods, to explain his *knowledge* of the 'Thing,' and—pardon the uncouth word—the *thinghood* of that which is known; and how far, pray, will the explanation go in reliance on the conclusions of a strictly inductive and experimental psychology? The expert will have to stop short when he has enumerated certain principles that have respect to the quality, quantity, time-rate, and combination of the sensation-factors whose synthesis is the sensation-content of knowledge. But in doing this he has not explained, he has not even described with any approach to completeness, that state of consciousness which we call an act of knowledge. And here I am not asking of the psychologist a system of metaphysics or a theory of knowledge, to incorporate into his experimental resultant. But I am simply asking that he shall describe and explain, *as such*, that common enough state of consciousness which everybody calls 'the knowledge of a thing.' Nor does it seem to me at all likely that our physiological and psycho-physical laboratories will ever be able to handle certain factors and aspects of this psychological problem of knowledge. For example, how shall we experiment or collect statistics to elucidate the 'belief' in reality which different writers have assigned, now to intellection, and now to feeling, and now to will, but without which no knowledge of anything can take place? For my part, I am just as firm in my opinion as the most old-fashioned psychologist, while in admiration for the new psychology I yield to none, that self-consciousness envisages a self-activity, and a conviction of extra-mental reality, in all knowledge, which experimental data are quite powerless either to deny or to explain.

Nor do I look forward with much confidence to the elucidation of our so-called 'higher' æsthetical, ethical or religious sentiments, by experimental analysis or by collection of statistics. Something worth while will doubtless be done in the region of the simpler and more fundamental feelings, by labo-

ratory methods; and what is done in this region will help us the better to understand what happens in the higher regions of affective phenomena. But I suspect that the limitations of the successful use of these methods are likely to be pretty quickly reached; and that we shall have to go to art and to literature, as interpreted through our own best self-conscious feeling, for the clearer understanding of all such phenomena.

With respect to choice and free will so called, as elucidated by the modern experimental methods, my hopes are very moderate, and my fears are—*nil.* In this line of investigation it is quite too often forgotten what it is, taken in its depth and entirety, which needs to be described and explained. You may seat your reacting agent, tabulate and arrange your results, and conclude—we will suppose, for the sake of illustration—that theoretical determinism has received an experimental demonstration. But suppose that I, in common with the great majority of men in all ages, doubt the truthfulness of your conclusion; and that to your conclusion I oppose a certain conviction that sometimes, somehow, I determine instead of being determined,—a conviction which I also share with the great majority of mankind. Now this doubt and this conviction are themselves psychological facts; they are of no small import and of almost universal occurrence. But how are you going to investigate them experimentally; how describe, explain, or explain away, the doubt and the conviction, by psycho-physical methods? To be sure, you may tell me that, if a stone, which flies through the air to its predetermined spot on the ground, had a *plus* of consciousness added to its motion, it would be conscious of self-directed motion, in the absence of any knowledge of the laws of gravitation, pressure from atmospheric currents, etc. But here again suppose that I doubt; and perhaps revive the time-worn conviction. For I do not see why consciousness + motion should equal anything more than consciousness *of* motion; or why consciousness *of* motion + ignorance should develop doubt of determinism and conviction of freedom. But, since it is in no respect my intention to argue this ancient problem, I will conclude this point by returning to my main thought: I do not see how the hypothetical instance of a conscious machine enables us the better

to handle the aforesaid doubt and the aforesaid conviction, by the methods of the psycho-physical laboratory.

It is high time, however, to turn our attention to the other side of the relation we are discussing. A mere glance at this other side is sufficient; because I suppose there is not a member of this Association who does not approve of the study of mental phenomena by experimental and statistical methods. This country, following Germany and in marked contrast to Great Britain, has eagerly and—on the whole I am sure very intelligently and safely—adopted this method. Our larger universities have already equipped, or are rapidly equipping, themselves with psycho-physical laboratories; our smaller institutions even are demanding of their teachers some acquaintance, at least, with modern ways, and modern results, in the study of psychological science. All this is very stimulating, very hopeful. The expectation is not unwarranted that the United States will soon become the coworker, on equal terms, of the best European laboratories. It is not for purposes of flattery, but rather of warning, that I venture to say: The fate of this movement in this country will depend very largely upon the action, individually and in corporate fashion, of the members of this Association. For myself, within limits which I have already roughly and inaccurately sketched, I look for a large development of the science of psychology, in the near future; and I am certain that this development will not be without influence upon the current philosophy and theology, as well as upon the practical welfare of the people. This confidence has its principal reasons in the necessarily close relations that exist among all the subordinate departments of the science of psychology, and the especially intimate relations in which the science stands to philosophy and to the life of conduct and the development of character. This last remark brings me to the second of the three points which it is my purpose to consider.

Philosophy is on the whole much older and more interesting to the human mind than is the science of psychology. Indeed, philosophy is older than any science, whether of mind or of matter. Various definitions setting forth different conceptions of philosophy have been put forth at different

epochs in its development. Perhaps the chief characteristic of the modern conception has reference to the relation in which philosophy stands to the various concrete or particular sciences. A passing glance at the way in which the present more cordial understanding of the two has come about may fitly be given; for here, as everywhere, the history of the evolution of human knowledge is full of instructive lessons. We go no farther back than to recall how the most stupendous systems of speculative thinking were built on ground which had been apparently swept quite bare by the criticism of Kant. This 'astounding' thinker, as Schopenhauer has called him, supposed that the negative result of his labors would be to remove forever the pretence of ontological *knowledge*, while 'making room'—to use his own phrase—for faith in the verity of certain postulates respecting ethical and religious entities. Much has been written concerning the failures and successes of the Kantian criticism, and concerning the causes of both. In my judgment—although I speak somewhat diffidently, because I am not aware that any of the most distinguished critical students of Kant have put the matter in just this light—the chief cause of the failures of this greatest of all modern thinkers lay in his imperfect and wrong conceptions of a psychological sort. Kant did not understand in a scientific way the common consciousness of the race. Especially defective and erroneous is his conception of *knowledge;* I do not now mean his theory of knowledge, but his descriptive history and implied analysis of that state of consciousness which all men recognize as entitled to be called 'knowledge.' But without proving this charge, and not to be drawn too far aside from the main current of my intention,—the issue showed that men would not be warned off by a critical theory of cognition from the 'pretence' of ontological and systematic knowledge. And, indeed, how could they be; since there is no such thing as knowledge that is not ontological? 'Ordinary' *knowledge* and 'scientific' *knowledge* are as full of unverifiable postulates as were the old-fashioned rational psychology or the rational theology; the only net, valuable result of no end of criticism being to discover what postulates, or fundamental faiths, enter into

all knowledge, and how they may be so understood and expressed as best to hang together.

Now, contemporaneously with the strong reaction against the negative conclusions of the Kantian criticism, in philosophical circles, there went on a mighty forward movement of the physical sciences as pursued by the more strictly inductive method, with the determination to prove all speculative hypotheses by experimental tests, and to express such of them as stood the testing in the intelligible and accurate terms of mathematical formulas. It was inevitable that these two movements should have a somewhat varied and sometimes painful experience in the effort to adjust relations with each other. In my opinion, if we set aside the theologians, the students of philosophy have on the whole behaved far better than the 'scientists' so called. I am not aware that even Hegel anywhere manifests a contempt for facts, as such, or flouts at the conclusions of his contemporaries in physical science, so far as he understood them to be scientifically derived. He undoubtedly everywhere manifests an overweening confidence in his ability to give an ultimate explanation of all these facts in accordance with the method and principles of the dialectical philosophy. But Mr. Herbert Spencer has not half as much expressed horror for the merely abstract, or manifest eagerness to get at the heart of the concrete and the real; and perhaps Hegel, when he steps over too far upon the domain of the positive sciences, is not—making allowance for the condition of things in his day—any more ridiculous than some of the modern disciples of science have been when they have transgressed the limits of their specialties (for example, Mr. Huxley in the arena of biblical criticism).

For an entire generation, which now seems happily drawing to a close, the relation of philosophy to the positive sciences, or rather of these sciences to it, was one of open antagonism or half-concealed contempt. 'Metaphysics'—for so all branches of philosophy were often sneeringly called—was a tabooed subject for the student of physics. And yet what was actually going on all this time? Why, within the domains of physics, chemistry, and biology, a system of 'meta-

physics' was being evolved which, although it does not know itself by that name and rarely arrives at an adult stage of self-consciousness, is quite as wonderful and stupendous in respect of its postulates as were any of the philosophical systems which followed the criticism of Kant. Nor will it do to maintain that this underlying and interpenetrating ontology can be removed and the modern system of the physical sciences remain, as sciences, in the same condition as before. The rather is it true that the complete removal of this metaphysical system would reduce the sciences from the condition of knowledge to the mere pretence of knowledge; from the claim to be systematized truth about real things and real events to mere *Schein*, as it were. Indeed, if it were in the line of my present pursuit to do this, I think it could be shown that the only result of the consistent carrying-out of this negative criticism and the resulting agnosticism is the falling in one common ruin of the rational foundations of daily conduct, of the natural and physical sciences, of ethics, and of theology—of the whole temple of human *knowledge*.

Of late and for some time, however, there have been plain signs that the age of opposition and conflict between science and philosophy is being replaced by an age of 'reconciliation.' Indeed, 'reconcilers' of science and religion, of science and philosophy, of philosophy and religion, are everywhere, thicker than bees in the blossoming-time of a Southern spring. Plainly, it is reconciliation which is in the air; and he is an 'old fogy' in spirit, however youthful he may be in age or appearance, who continues to talk with Haeckel about "strangled snakes lying around the cradle of the young Hercules"—namely, modern science—and other high-sounding but ill-timed phrases to the same effect. Better and wiser, by far, and at once more philosophical and more scientific, to hold out the hand, with Helmholtz and Du Bois-Reymond, or with Lotze and Herbert Spencer, towards the other party, to whichever of the two one happens to belong. For my own part, I have no great confidence in the permanency of the actual reconciliations thus far effected; and this both because they have been made for the most part by men of only second-rate quality, and also because they have been quite too super-

ficial in the selection of subjects on which to make the attempt at reconciliation. But the spirit is admirable; and good results cannot fail to follow in the near future.

For science and philosophy will always exist; neither can expel the other from the region of human interests and human endeavor; indeed, no rigid demarcation can ever permanently divide them; each will flourish only in dependence upon the other. Moreover, minds whose interest is chiefly in facts and whose skill discovers itself chiefly in collection of, and lower generalizations from, facts, will always exist; nor will the race of other minds cease whose interest leaps forward toward the places from which to survey the more ultimate meanings of the facts, and whose greatest skill shows itself in the wider speculative treatment of them. And occasionally great minds will be vouchsafed to the race, who will combine the ability to acquire a large amount of scientific data of various kinds with skill in philosophical analysis and a genius for philosophical synthesis; and these minds will be among the greatest benefactors of mankind with respect to the development of both science and philosophy. For philosophy is but wild and mischievous speculation, unless it build itself upon the concrete and particular sciences; and science is but the unsatisfying husk of knowledge, is without rational self-consciousness and highest import and divinest interest, unless it intelligently lend itself to help, and to be helped by, philosophy.

But of all the particular sciences it is psychology which stands in the most intimate relation to philosophy. We are in this day making an attempt, both valiant and in large measure wise, to separate between the science of mental phenomena and those metaphysical assumptions which have hitherto so largely overlaid and suppressed the growth of the science. In the interest of this separation we are told that a mixture of metaphysics and psychology as 'a natural science' spoils both ingredients, and, as a mixture, is apt to please neither of the two classes of patients for whom it may be supposed to be prepared and prescribed. This is true; although the truth depends chiefly upon the proportion of the ingredients which enter into the mixture.

Yet there is another side to all this which we cannot afford

to lose out of our total account. We have seen that it is difficult to pursue any form of a so-called 'natural science' without being called to consider, philosophically, its principles, their import, and their relation to the principles of other more or less closely allied sciences. The moment we begin to strive for a knowledge of principles, however, we come perilously near to the border-line—all invisible as it is—between science and philosophy. But, however this may be in the case of the physical sciences, there can be little doubt that the relations between psychology and philosophy are much more intimate. They are *so* intimate, indeed, that many of the most profound students of both, approaching them from varying points of view, have declared it to be impossible to separate between the two. It is well known that Herbart—to whom, in spite of his many errors, the modern science of psychology owes an enormous debt—declared: "The whole series of the forms of experience must be investigated twice over, once metaphysically and then again psychologically;" although he adds that these investigations must lie "side by side" and be compared so that we may never again confuse them. His most distinguished disciple, Volkmann von Volkmar, whose work on psychology, although I differ *in toto* from many of its conclusions, seems to me the most mature and magnificent of modern times, declares it to be impossible to separate between psychology as a science of mental phenomena and rational psychology, or the philosophy of mind. While Wundt, who differs very widely in method and conclusions from the Herbartians, affirms that the relation of psychology to philosophy is so close and peculiar that the partition of sovereignty between the two is an abstract scheme which, in the presence of actuality, must always appear unsatisfactory.

It is both significant and amusing to notice the actual behavior of many who theoretically deny that any such intimate relation must be acknowledged. For example, Höffding, in his most interesting and excellent treatise on psychology, after formally announcing that he proposes to treat the subject solely in a scientific way, almost immediately, and without waiting to marshal his facts, declares philosophical monism to be the only tenable view of the relations of mind

and body. And it is not long since I read in a magazine article, designed to set forth results in experimental psychology, the astonishing statement that no truly 'scientific' (*sic*) psychologist in these days held any other view on this question than the monistic; it would seem, then, that all dissenters on philosophical grounds from the modern Spinozism are to be read out of the ranks of science by this ardent young brother.

Now I do not mention these two classes of opinions for either confirmation, denial, or debate. That a worker in psychology may conduct an elaborate series of experiments, or discuss some important psychological principle, without once raising the questions in discussion between monism and dualism, materialism and spiritism, there can be no doubt. But surely psychology has as much right as has physics to its speculative hypotheses and supreme generalizations, if only these are kept in their place of close dependence on observed facts and sound reasoning. Much more than this is, in my judgment, however, true. For the relation of psychology, as a science, to the philosophy of mind, and through it to all philosophy, is so intimate and binding that not one of the larger psychological problems can be thoroughly discussed without leading up to some great debate in the field of philosophy. As long as psychology is naturally propædeutic to philosophy every one must be puzzled to tell just when he has crossed over the line and left the plain paths of science behind in order to get lost in the jungles of metaphysics.

If time permitted, this general statement might be enforced with almost indefinite detail. Thus it is exceedingly difficult, if not impossible, to give a thorough psychological discussion to the phenomena of perception by the senses without taking some position—at least an implied one—respecting the philosophical questions in debate between Realism and Idealism. Who would willingly undertake to separate strictly between the psychology as 'natural science' and the psychology as philosophy that are involved in the Empiricism of Wundt and Helmholtz, and in the Nativism of Dr. Ward or of the exceptionally admirable treatise of our own Professor James? This relation of well-nigh inseparable intimacy must continue to exist, and it will survive all warnings and all ex-

hortations; because it is not, in the last and supreme and most difficult effort, some account simply of the intensity and content and time-rate of sensations which psychological science has to render; it is rather of the faiths and fears and opinions and knowledges of mankind about things. And as the late Professor Croom Robertson said: "We *may* view knowledge as mere subjective function" (that is, psychologically); "but it has its full meaning only as it is taken to represent what we may call objective fact, or is such as is named (in different circumstances) real, valid, true." But he goes on to say: "Philosophy, on the other hand, is the theory of *knowledge* (as that which is known)." Yet again, we agree further with Professor Seth when he says: "It is evident, then, that philosophy as theory of knowledge must have for its complement philosophy as metaphysics or ontology." Putting all these declarations together, what can be made to follow besides the obvious proposition that a full-orbed science of psychology is propædeutic to and implicative of both epistemology and ontology?

This Association is formed in the interests of a science of psychology; it cannot therefore be expected to occupy its time and energies largely in the discussion of philosophical problems. Its members, however, would be something either more or less than completely human if they took no interest in any of these problems. The preceding remarks have been designed to introduce the exhortation that, since, from the very nature of our science, we shall scarcely always be able to avoid all seeming of entanglements more or less epistemological and ontological (and perhaps even ethical and theological), we should, first, add the philosophical spirit to our scientific intent; and, second, be not only wise and cautious, but also tolerant and generous toward the various possible expressions of philosophical views.

A few words will suffice for suggestions on the third of my three points; since—as has already been said—no considerable divergence of opinion is to be anticipated here. It is reasonable now to be very enthusiastic concerning the contributions which a scientific psychology may be expected to make toward the practical welfare of mankind. This fact seems to

me to place a certain weight of responsibility, which is of a quasi-ethical sort, upon such an Association as ours. It is sometimes supposed that the truly *scientific* spirit and attitude require a man to be interested in science solely for science's sake; or—to put the case in a yet more captivating way—in scientific truth for this truth's sake. Now I should not willingly be inferior to any one in devotion to the truth for 'its own sake' (as we are wont to say); and I trust that I have a sufficient admiration for the scientific spirit and for the splendid triumphs of modern science so called. At the same time my observations lead me to admit that not a few who cry most loudly in the name of 'science' show quite too plainly that it is chiefly for their own sakes; nor do I find that it has been the thing of smallest import with the truly great men in science that their pursuits enabled them to be, in no small measure, benefactors of mankind. And while they have been more unwilling than ordinary men to swerve a hair's breadth consciously from the strictest truth, and have had a generous confidence in that blessing which adherence to the truth brings, they have also recognized that the highest and truest truth which it is given us men to know, somehow seeks and finds an embodiment in conduct and character.

For example, astronomy, being originally devised in the interests of humanity as astrology, and then becoming truly scientific, has returned far more than all its costs as navigation, meteorology, natural theology so called, etc.

A fortiori is the obligation to be of practical benefit heavily laid upon psychology. The more I study and teach this science, the deeper does the impression become that it is able and destined to contribute greatly to the welfare of mankind. I shall now close these remarks with a brief enumeration of some of the well-known directions in which we all hope to see this impression realized.

First: the science of psychology may be expected to make large contributions toward the improvement of the art and practice of teaching. Pedagogics so called is already a considerable 'fad' in this country. It is, however, I assure you, something far different from the contempt born of professional pride which leads me to say that, with comparatively few

exceptions, the written and oral work on this important subject, in America, is shallow and misleading to an almost incredible degree. Meantime, an enormous waste—amounting to some three or four years in the ten or twelve of our public-school life, on the average, for each one of the millions of our school-children—is ceaselessly going on. Several causes combine to bring about this deplorable result; the most complete cure possible can, of course, be effected only by dealing with all these causes. But one of the most important helps to improvement must come through the instruction of the teachers of these children in the principles of a truly scientific psychology. And such instruction must emanate from the highest expert sources, and penetrate to the lowest strata and the remotest regions in the public-school system. It can never come in the form of half-baked treatises put forth by writers who, however seemingly successful they may have been in practice, have no scientific understanding of the principles on which even their own—too often merely apparent—success has been based.

Again, the science of psychology may be expected to contribute much to the science and practice of medicine,—especially, of course, in the department of neurology. Even modern surgery has already been guided by the help which physiological psychology has rendered in the discoveries, since 1870, in the localization of cerebral function. Looked at from a truly rational point of view, what can be more amazing than the fact that thousands of doctors are to-day treating patients suffering from 'mental' disease, who themselves never made the slightest study of mental phenomena, sane or abnormal, in any scientific way? With so many quacks, on the one side, medicating the mind with drugs, is it greatly to be wondered at that there are so many cranks on the other side who are advocating the treatment of all disease with 'mind-cure' or 'faith-cure'? In my opinion the time will come when no reputable medical school will think of giving a diploma to a student who has not made a thorough study of psychology, at least as far as its elements may be pursued from the physiological and experimental points of view.

In the diagnosis and treatment of the insane, the incorrigible, the idiotic, etc., scientific psychology is surely destined

to exert a growing influence. In time it will come to appear that the student of anthropology, of criminology, or of sociology, who has failed thoroughly to lay his foundations in the modern psychology has been guilty of an oversight or neglect fatal to his highest success. Nor will certain forms of jurisprudence long continue to disregard their natural relation to the scientific study of mental phenomena. As civilized nations come to distinguish between the man who is fit to be a 'keeper' of the insane or the criminal, and the man who is fit to give expert testimony to distinguish between the insane and the sane criminal, the advantages of prolonged psychological investigation for the improvement of jurisprudence will be more clearly discerned.

But there is no need to specialize further. In general, why should we not expect to see our science contributing to the improved conduct and character of men, in the school, in the court-room, the prison, and the asylum? Nay, I am not without hope and expectation that even the sacred offices of the religious teacher, as well as the no less sacred offices of the teacher of ethics by parental influence—of the mother whose breasts with their stores of nourishment, and face-to-face intercourse with the infant according to principles which regulate the earliest sensory-motor and imitative functions, fix the lines of behavior and of destiny—may all be helped and blessed in no small degree by the recent rapid advances of human psychology. And this is the chief reason why I close, as I began, with words of cheering reminder that this Association should enter upon its career with a sufficiently generous estimate of its privileges and of its responsibility.

THE CASE OF JOHN BUNYAN. (I.)

BY PROFESSOR JOSIAH ROYCE,

Harvard University.

THE casuistry of the numerous forms of insistent mental processes of a pathological character has of late years become very extensive. The names and sub-classes of these morbidly insistent kinds of feeling, thought, or volition have occasionally been multiplied beyond any reason, until, in view of the endless '*manias*' and '*phobias*' that some writers have been disposed to dignify with special titles, I myself have sometimes wondered whether it would not be wise for some one, in the interests of good sense, to try to check this process by defining, as a peculiarly dangerous type of insistent impulses, a 'new mental disorder,' to be described as the 'mania' for multiplying words ending in *mania* or in *phobia*. Meanwhile, despite this inconvenience, and despite numerous hasty speculations upon the whole subject, there can be no doubt that the theoretical interest of these morbidly insistent mental processes is great, and that the pathological secret and the genuine natural classification of these disorders will be such as well to repay the trouble of the most minute study of cases, if only that secret ever comes to be made out, and that natural classification is ever set up. And while we wait for further light, the careful preliminary scrutiny of cases is indeed the only course open to students of psychology.

The present paper is but a very modest contribution to the casuistry of the morbidly insistent mental processes. I have no new *phobia* or *mania* to define, and in any case I speak only as student of psychology. The medical reader might be able to see much more in the documents to which I here wish to attract his attention than I am able to see. My task is simply one of summary and report. The case to which I wish to call

attention is meanwhile one of peculiar interest, namely, that of the author of the *Pilgrim's Progress*. The principal document concerned is John Bunyan's remarkable confession, entitled *Grace Abounding to the Chief of Sinners*, an autobiographical statement which Bunyan wrote and published, as the title-page tells us, "for the support of the weak and tempted people of God." This little book is, from the literary point of view, of very high interest, ranking, as I suppose, amongst all the author's works, second only to the great *Pilgrim's Progress* itself. As a record of human experience, the *Grace Abounding* will never lose its charm, both for lovers of religious biography, and for admirers of honesty, of sincerity, and of simple pathos. Nothing that can be said as to the psychological significance of the author's recorded experiences will ever detract from the worth of the book, even when viewed just as the author viewed it, as a 'support' for the 'weak and tempted.' Bunyan, as we shall see, had at one time a decidedly heavy and morbid burden to bear. But, like many another nervous sufferer of the 'strong type' (Koch's *starker Typus*), Bunyan carried this burden with heroic perseverance, and in the end won the mastery over it by a most instructive kind of self-discipline. In view of this fact, a clearer recognition of the nature of the burden, from the psychological point of view, rather helps than hinders our admiration for the author's genius, and our respect for his unconquerable manhood. It is this sort of case, in fact, that renders the study of the nervous disorders so frequently associated with genius, a pursuit adapted, in very many instances, not to cheapen our sense of the dignity of genius, but to heighten our reverence for the strength that could contend, as some men of genius have done, with their disorders, and that could conquer the nervous 'Apollyon' on his own chosen battle-ground.

But an estimate of Bunyan's genius belongs not here. I venture only to say that I write as an especially profound admirer of this wonderful and untaught artist, whose homely style shows in almost every line the born master, whose simple realism in portraying human character as he saw it amongst the live men about him often puts to shame the ingenuity of scores of cunning literary craftsmen in these our

own most realistic days, and whose few highest flights of poetic imagination, such as the closing scenes of the first part of the *Pilgrim's Progress*, belong without question in the really loftiest regions of art. Range of invention, self-control in production, perfect objectivity in the portrayal of human life,—these are leading traits in the work of this man; and these things, as well as others that we shall later see, forever forbid our classing Bunyan, taken as a whole, amongst the weaklings. It is perfectly consistent with this fact, however, when we find this admirable man and artist living, for a bitter and instructive period of his early years, a life of stern conflict with a nervous foe of a fairly recognizable and, under the circumstances, decidedly grave type. How, unaided and ignorant, he won the victory, is in itself an interesting tale. And, for the rest, the case tends to throw light on the interesting problem as to how far the presence of elaborate insistent mental processes of a morbid type is of itself a sufficient indication of the depth of the 'degeneracy' of constitution of the subject who, is for a time burdened with them.* That Bunyan's malady must have had a certain constitutional basis will, I suppose, appear decidedly probable to most readers of the following summary. Yet it will be hard to question the fact that, quite apart from his special creative abilities, Bunyan's general constitution,—his extraordinary and persistent power of work, his long endurance of very serious mental and physical hardships, his reasonably lengthy life of sixty years (ended by an acute disease, due to an exposure), his apparently even temper and self-possession in later years, his sustained influence over men as leader, adviser, and preacher,—when taken all together, must give us an idea of his inherited organization that will, in any event, stand in a fairly strong con-

* The frequent association of the morbidly insistent processes with the nervously 'degenerate' type is a commonplace in the literature of the subject, and a few years since it was, I believe, an almost if not quite universal dogma that considerable masses of insistent fears, impulses, or thoughts occurred only as part of the 'stigmata' of degeneracy. The possibility of the development of even elaborate systems of such insistent impulses upon a basis of wholly acquired neurasthenia was maintained by Dr. Cowles, in his well-known paper on *Insistent and Fixed Ideas* in the *Amer. Journal of Psychology* (vol. I. p. 222 sq.), and has also been asserted by others.

trast to the impression that the temporary nervous disorder of his early manhood, if it were taken alone, would leave upon our minds.

But a deeper estimate of such things I must leave to more competent judges. I have here only to present the facts.

I.

John Bunyan was born November 30, 1628, and died August 31, 1688. The principal known facts of his life which bear in any way upon the question of his health and constitution, apart from the narrative in the *Grace Abounding*, are as follows:* Bunyan was a native of the little village of Elstow, near Bedford. His family can be traced in Bedfordshire as far back as 1200. In the sixteenth century, an ancestor of Bunyan, and the wife of this ancestor, appear in court records as brewers and bakers. Thomas Bunyan, his grandfather, was 'a small village trader.' Difficulties in the courts are the occasion of some of the records preserved of these ancestors, but the difficulties named are petty, e.g., minor violations of excise laws, disrespect to churchwardens, and perhaps religious nonconformity.† Bunyan's father was notoriously, like Bunyan himself, a 'tinker' or 'brasier,' probably, says Brown, "neither better nor worse than the rest of the craftsmen of the hammer and the forge." Tinkers had, to be sure, in that time and place, a reputation as rather hard drinkers; but on the other hand they wandered much on foot, and so lived freely out of doors. Bunyan's father lived until 1676, dying at seventy-three years of age. The poet's mother was of a poor but very honest and thrifty family; she died when John Bunyan himself had reached the age of fifteen. Little more is known of the family before we reach our poet himself. He was not an only child. One sister is known to have died early. One brother is known to have lived until 1695.

* I use, for the most part, the principal recent biography, that of John Brown (2d edition, London, 1886)—an elaborate and extremely patient research into every discoverable detail relating to Bunyan's family and fortunes. Other recent accounts are those of Venables (in the 'Great Writers' series, London, 1888) and of Froude (in the 'English Men of Letters' series). The ground has thus been very thoroughly gone over, for all literary purposes, in recent years.

† Brown, pp. 27–31.

Of John Bunyan's childhood history we shall see a little soon. In youth he was apparently, until after the time of his marriage, of pretty lusty health. The 'wicked' early life of which he speaks so severely in his *Grace Abounding* proves, on the whole, to have been, physically speaking, a wholesome life, during all the time preceding his conversion. Alcoholic excesses and unchastity are, in the opinion of all his modern biographers, nearly or quite excluded by what we most certainly know of him at this time. At about sixteen years of age Bunyan was enrolled in the army, probably on the Parliamentary side, and remained some two years in service, but apparently without any physical ill effects. He married at twenty years of age, both himself and his wife being very poor. He now followed his trade as tinker. Within the next four years fall, first his conversion, and then the experiences of which we are principally to speak in what follows. In these years, furthermore, falls also the birth of his first child, a daughter who was very early blind. In 1653, after he had passed through these principal experiences, he joined the church in Bedford. In 1654 his second child was born, also a daughter. In 1655 he began that career as preacher which he continued thenceforward, so far as he was permitted to do so, until the end. In 1660 he was imprisoned in the county jail at Bedford, for violating the law by acting as an irregular preacher; and there he remained, in a confinement which varied in its degrees of strictness, for some twelve years. The physical strain of this imprisonment must have been great, and the mental anxieties involved were of the severest, as we learn from his own account; yet Bunyan plainly experienced no return of his previous mental troubles with anything like their old force. He was now often weak in body and depressed in mind, but never long despairing. He busied himself both in preaching to his fellow-prisoners and in writing. He was released in 1672. For three years thereafter he was at liberty. In 1675–6 he suffered a second imprisonment, during which it was, according to recent research, that he wrote the *Pilgrim's Progress*.* Thenceforth he continued working as writer and preacher to the end.

* Brown, p. 254; Venables, p. 151.

The list of his works contains 'sixty pieces,' says his first bibliographer, 'and he was sixty years of age.' One standard edition occupies four volumes octavo. His works are, of course, largely theological. They are certainly laborious productions, even apart from the genius involved; for this man was never trained to write.

As to his health otherwise, we know that, after 1653, there was a time in his early life when, as he says, "I was much inclining to a consumption, wherewith, about the Spring, I was suddenly and violently seized with much weakness in my outward man, insomuch that I thought I could not live." Other times, still later, he mentions, when he was 'very ill and weak'; and he notes great depression of spirits as characteristic of his state at all such times.* Brown† holds, concerning Bunyan, that "at any time he was far from strong" as to physical health. But when one considers his remarkable activity both as writer and preacher, and the long and severe strains to which he had been subject before he reached sixty years of age, and when one remembers also the possibly hypochondriac nature of the disorders of which his own account, as just cited, speaks, it seems hard, after all, to form any exact opinion as to the actual degree of the physical weakness of his constitution. One is disposed to set the work done and the external sufferings endured over against the rather meagre record of later illnesses in his life. "His friend," says Brown (a friend, namely, who wrote an account of Bunyan), "tells us that though he was only sixty he was worn out with sufferings, age, and often teaching." One remembers hereupon that a persecuted genius who had written 'sixty pieces' without having received any sort of early scholarly training, and who had passed more than twelve years in unjust imprisonment, and all his life in struggle, had a right to be somewhat worn at sixty.

He died of 'a violent fever,' or, as others say, of 'the sweating distemper,' after having been exposed to 'heavy

* "The Tempter did beset me strongly (for I find he is much for assaulting the soul when it begins to approach towards the grave, then is his opportunity."—*Grace Abounding* (Clarendon Press Ed.), p. 375.

† *Op. cit.* p. 390.

rains and drenched to the skin' while on a preaching journey. Bunyan was twice married. He had in all three daughters and three sons. His first child, born during the time of his early disorder—a daughter—was, as observed above, blind, and died before him. Descendants of another of his daughters are the only descendants of Bunyan still known to survive. The later history of the family is incomplete, but, as reported by Brown, contains nothing of any note for our present purpose,—no record, namely, of remarkable disease or ability.

Of Bunyan's outward seeming, in his later years, we have two good accounts by contemporaries. One runs thus:

"As for his person, he was tall of stature, strong-boned, though not corpulent, somewhat of a ruddy face, with sparkling eyes; . . . his hair reddish, but in his latter days time had sprinkled it with gray; his nose well set, but not declining or bending, and his mouth moderately large; his forehead something high, and his habit always plain and modest. He appeared in countenance to be of a stern and rough temper, but in his conversation mild and affable, not given to loquacity or much discourse in company, unless some urgent occasion required it; observing never to boast of himself or his parts, but rather to seem low in his own eyes and submit himself to the judgment of others. . . . He had a sharp quick eye, accomplished with an excellent discerning of persons, being of good judgment and quick wit."

The other account speaks of his countenance as 'grave and sedate,' and of a sort to "strike something of awe into them that had nothing of the fear of God." The writer adds that his memory was "tenacious, it being customary with him to commit his sermons to writing after he had preached them." Bunyan's executive ability in church management and discipline is also noted in this account. As to his eloquence as a preacher, all accounts agree. This great 'dreamer,' then, was also, in his later years, a man of decided practical power, dignified in bearing, accustomed to control other men.

II.

So much, then, for the man as a whole. As to the experiences of his early manhood, recorded in the *Grace Abounding*, biographers in general have felt their perplexing intensity and abnormity, but have been accustomed either to refer them once for all to Bunyan's theological associations and ideas, or else to conceive them as indeed somehow pathological, but then to define their abnormal nature with the utmost looseness and confusedness.*

Patent, then, as are the reported experiences, beautifully as Bunyan confesses them, transparently as he unveils himself, one still has to go almost alone in trying to portray their actual connections; for biographer after biographer has passed these connections by with blindfold eyes. Yet the story, read in its psychological aspect, is as follows:

As a child Bunyan showed some of the familiar signs of the sensitive brain. He is not at all concerned, in his Autobiography, to gossip as to any minor matters. He tells us almost nothing of the externals of his life. He is wholly concerned in setting forth what God has done for his soul. He feels it worth while, however, to describe to us, in beginning the narration of his spiritual conflicts, certain of his early mental experiences. In childhood, so we learn, his 'cursing, swearing, lying, and blaspheming' were very marked faults. To quote his own words: "So settled and rooted was I in these things, that they became as a second Nature to me. The which, as I have with soberness considered since, did so

* Macaulay, for instance, in his *Miscellanies*, declares that, at a certain point, Bunyan's mind began to be 'fearfully disordered'; but he then proceeds, with a very undiscriminating analysis of the data, to define Bunyan's mental symptoms so that, if this analysis were sound, they would make up a case of what we should now define as 'hallucinatory delirium.' This Bunyan's disorder very certainly was not, in any fashion whatever. Taine, who, as psychologist, should have seen more clearly, is, in his way, (in the account of Bunyan in the *English Literature*,) almost equally confused as to Bunyan's true temperament and condition, and even imagines the calm and self-possessed art of *Pilgrim's Progress* to be the outcome of the 'inflamed brain' whose sufferings are depicted in the *Grace Abounding*. But the Bunyan of 1650 was not yet the Bunyan of the *Pilgrim's Progress* of 1675. Venables and Brown, well as they summarize the salient facts, fail to see their psychological significance. Froude also appears to go wholly astray in this respect.

offend the Lord, that even in my Childhood He did scare and affright me with fearful Dreams, and did terrify me with dreadful Visions. For often after I had spent this and the other day in sin, I have in my Bed been greatly afflicted, while asleep, with the apprehensions of Devils and wicked Spirits, who still, as I then thought, laboured to draw me away with them, of which I could never be rid." To these persistent nocturnal terrors there were added still other and evidently often waking troubles, 'thoughts of the Day of Judgment,' which gave him fears and 'distressed' his 'soul,' 'both night and day,' so that "I was often much cast down and afflicted . . . yet could I not let go my sins." These experiences came "when I was but a child, nine or ten years old." "Yea," he adds, "I was also then so overcome with despair of life and heaven, that I should often wish either that there had been no Hell, *or that I had been a Devil*—supposing they were only Tormentors; that if it must needs be that I went thither, I might be rather a Tormentor, than tormented myself." Of such early sufferings we have several accounts besides the foregoing summary statements.

Childhood experiences of this sort have to be estimated as important in direct proportion to their depth and in inverse proportion to their dependence upon the suggestions to which a given child is subjected. These dreams were, plainly, in some instances very elaborate and detailed. Bunyan's later youthful ignorance, so freely confessed, concerning all theological matters indicates, however, that these fears and this despair were no part of any very coherent system of childish thoughts on religious topics. The content of his 'terrible dreams' was of course derived from what he heard at church and elsewhere; but a sufficient basis, in these suggested ideas, for such marked trouble seems very improbable. That the nocturnal terrors and the despair were in part primary symptoms of nervous irritability, one can thus hardly doubt. As to the depth of the experiences themselves, the very fact of Bunyan's careful report of them is, under the circumstances, convincing. For his Autobiography is, as has just been noted, extremely reticent as to all matters that he does not consider essential parts of the tale of God's dealings with his soul.

In youth, at what seems to have been the healthiest period of his life, these dreams left him, and were "soon forgot . . . as if they never had been." And now began the wilful and sinful time which Bunyan later so unsparingly condemns. That his sins did not include unchastity or drunkenness seems, as aforesaid, clear to all his recent biographers, and for good reasons too, into which I need not here enter. Bunyan was now a very active and daring lad, who, in his almost complete ignorance, as Froude and others have observed, had no other way of expressing his genius than by "inventing lies to amuse his companions, and swearing they were true" (Froude's expression), and by showing extraordinary ingenuity as the chief swearer and wild talker of the village, so that even 'very loose and ungodly' wretches, as Bunyan tells us, were shocked by the flood of bad language in which this still unconscious poet was moved to voice his latent powers. These offences, and the still worse crime of playing tip-cat on Sundays, abide later in Bunyan's memory as evidences of the depth of his lost condition during these days. Meanwhile, despite the vulgarity of his surroundings and the restless waywardness of his life, Bunyan would otherwise appear to have been, on the whole, an exceptionally pure-minded youth. His early education, obtained in a local school, was extremely meagre.

His boyish marriage must have involved serious responsibilities. He and his young wife had at first not 'so much household stuff as a Dish or Spoon' between them. But the wife, 'whose Father was counted godly,' had, as her inheritance from this now dead father, two religious books, which Bunyan read with her, yet, so far as he was concerned, without 'conviction.' But ere long these books and his wife's speech 'did beget within me some desires to religion,' and for a while Bunyan attended church busily, 'still retaining my wicked life,' but already feeling some doubtful concern as to his own salvation, and much admiration for the formal side of church worship. A sermon against Sabbath-breaking brought him his first 'conviction.' After service and dinner, that day, when his full stomach had made him already cheerfully forget his transient remorse, he went, as usually on Sunday after-

noons, to play his game of cat. But having struck the cat one blow from the hole, "just as I was about to strike it a second time, a Voice did suddenly dart from Heaven into my Soul, which said, *Wilt thou leave thy sins and go to Heaven, or have thy sins and go to Hell?* At this," he goes on, "I was put to an exceeding maze. Wherefore, leaving my Cat upon the ground, I looked up to Heaven, and was as if I had, with the Eyes of my understanding, seen the Lord Jesus looking down upon me, as being very hotly displeased with me, and as if he did severely threaten me." The result of this sudden internal vision, of which he said nothing to his comrades, was an immediate sense of his general sinfulness, an overwhelming despair, which kept him standing 'in the midst of my Play, before all that were then present,' until, with a swift dialectic characteristic of all his later experiences, he had reasoned out the conclusion that it was now too late, since he had sinned so much, and that the only hope was to go back to sin, and take his fill of present sweets. "I can but be damned, and if I must be so, I had as good be damned for many sins as damned for few." He thereupon went on with the game, and in the immediately subsequent days swore, played, and 'went on in sin with great greediness of mind.'

The automatic internal vision seen with 'the eyes of the understanding,' but seen more or less suddenly, with extraordinary detail and with strong emotional accompaniment, appears henceforth as a frequent incident in Bunyan's inner life, and later became, of course, the main source of his peculiar artistic power. He was plainly always a good visualizer. But this automatic organization of his images was an added characteristic of the man, and an invaluable one. This 'power of vision' remained, as the *Pilgrim's Progress* itself shows, late in life; and without it our 'dreamer's' genius could not be conceived. In his times of depression these visions, in later days, took on the shading of his mood; but in themselves they were of course signs, not of depression, but of poetic power. Apart from other and serious causes of disturbance they plainly never approached near to any hallucinatory degree; and Bunyan always describes them so as to distinguish them

clearly from hallucinations, even when his condition, as described, is one of great agitation.

Shortly after this time the reproof of a neighbor again startled Bunyan from his reckless ways, and he resolved to begin in earnest the work of reform. The result was a period of a year (or probably somewhat less), during which he undertook nothing less than a systematic course of conscientious self-suppression. He 'left' his swearing at once, and in a way that astonished himself. He gave up his games as vain practices; after a long struggle he even abandoned dancing. He read the Bible; he lived a life of reform that astonished his neighbors; "for this my conversion was as great as for Tom of Bethlem to become a sober man." Inhibition of all outwardly suspicious deeds became the one rule of his life. He still wholly lacked what he later regarded as true piety, and he indulged in some spiritual pride in view of the approbation of his neighbors; but he cultivated a painful scrupulosity. We can well conceive how the material cares that beset this very poor but now married youth, and this sudden change from a careless life, of numerous relaxations, to an existence wherein every act was a matter of scruple, and wherein the opinions of all his neighbors were now so much taken into account, must have involved a considerable strain. The immediate consequences were characteristic of the whole case.

(*To be continued.*)

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY. (I.)

BY PROFESSOR HUGO MÜNSTERBERG.

A. MEMORY.

(With the assistance of Mr. J. BIGHAM.)

The experimental study of memory, important both for psychology and for pedagogics, is as yet only begun. The only experiments we have are those of Ebbinghaus, which cover simply the question of the influence of repetition and of the time-interval, made with but one material (syllables), and with only one subject*. Many other questions arise if we wish to understand the mechanism and the conditions of reproduction and memory. We began our systematic study of these questions by an investigation the purpose of which was to determine the action of disparate senses in recollection, especially to discover whether the different senses act at the same time independently, or help, or hinder each other. Collateral questions as to the influence of various modes of presentation, of various content, etc., could be answered by the same experiments. Studies on the reproduction of more complicated content, on the influence of the filling of time-interval, etc., will be reported later.

The simple apparatus used in the experiments consists of several series of ten small squares of paper of different colors, and of several series of black numbers, each one mounted upon a square white card of the same size as the colored squares. For audible presentations the names of the colors or numbers were spoken by the conductor of the work, while for visible presentations those colors and numbers were exposed upon a black background. The subjects were supplied with several corresponding series of colored squares ($3\frac{1}{2}$ cm) and

* The experiments on memory by G. E. Müller and F. Schumann (Zeitsch. f. Psychol. Nov. 1893), published while this article is in press, have in like manner no direct relation to the present investigation.

mounted numbers. After learning the presented series the subjects arranged these small numbers and colored papers in the order of the given series as these were recollected. The purpose in using numbers and colors was to secure a presentation-content as free from associations as possible. The length of the series consisted of either 10 or 20 presentations. Two sets of digits and colors were used, so that the same presentation might be given twice in a series even of ten presentations. The content of each series might be entirely audible, entirely visible, or partly audible and partly visible; it could consist wholly of numbers, wholly of colors, or of numbers and colors arranged in groups, in pairs, or in alternation. Thirty-two different kinds of series were employed: 20 successive audible numbers, 20 successive audible colors, and 10 successive colors alternating with 10 numbers (for instance: red, 6, gray, 2, green, 0, yellow, 7, etc.), 10 successive audible numbers followed by 10 successive audible colors, or the reverse, etc., 20 successive visible numbers, 20 simultaneous visible numbers, 20 successive visible colors, 20 simultaneous visible colors, 10 successive visible colors alternating with 10 successive visible numbers, 10 visible colors alternating with 10 visible numbers all presented simultaneously, etc., etc., 10 successive colors audible followed by 10 successive numbers visible, 10 numbers visible followed by 10 colors audible, 10 numbers visible alternating with 10 colors audible, 10 successive colors visible and 10 numbers audible presented in pairs (number given while color is seen), 10 successive colors visible and their names heard at the same time, etc.—altogether, 32 combinations in perfectly symmetrical arrangement. The time taken for learning each single presentation was 2 seconds, therefore 40 seconds for a series of 20 presentations, seen or heard, simultaneously or successively presented. The subjects saw or heard the series only once; the recollecting was done immediately at the conclusion of each single series. The subjects were cautioned against the use of mnemonic devices, and were informed beforehand of the length, the character, the content, and the mode of presentation of each series. Five subjects of the average age of 24 years took part, all making the same experiments,—each man working fifty hours, during

the winter of 1892–93. Every day a few short series were used as preparatory practice; these were not recorded. Provision was made for resting the subjects. To avoid any disturbance by mere training, each person made only two experiments at a time with each of the 32 series. All the results which are to be compared came, therefore, under the same conditions of practice, training, and fatigue. The errors were recorded solely as displacements of the single presentations in each series; blue-red instead of red-blue were therefore two errors, so that in a series of 20 presentations 20 errors were possible.

We shall first consider the 32 kinds of series only as audible, visible, or mixed, and disregard all other elements in the experiments. We add for each of the five subjects the presentations of all the visible series and the errors made in them. The pure visible series offered 2140 presentations (colors or numbers); in these series A. P. had 365, Bu. 413, E. P. 479, etc., errors. If we take the percentages and calculate them in the same way for the pure audible series, the result is:

Visible series—per cent of error: Bi. 18.7%, Bu. 19.3%, A. P. 17.1%, E. P. 22.4%, W. 25.1%—average. 20.5%.

Audible series: Bi. 34.1%, Bu. 31.4%, A. P. 25.1%, E. P. 35.9%, W. 31.6%—average, 31.6%.

With all the subjects the visual memory excels strongly the aural when they act independently. When sight and hearing act together as in the mixed series (excluding of course those series in which numbers or colors are seen and their own names heard at the same time), we have the following data:

Mixed series: Bi. 44.4%, Bu. 46.9%, A. P. 26.5%, E. P. 41.9%, W. 38.9%—average, 39.3%.

The memory for mixed series is therefore much weaker than for visible, and with one exception (A. P.) also weaker than for audible presentations. *When the two senses act together in recollection, they hinder each other.* But a special analysis of the mixed series can be made, to secure comparative tables of the audible and visible presentations which are variously combined in them. In the mixed series the percentages of errors in visible presentations are: Bi. 45.2%, Bu. 50.2%, A. P.

26.7%, E. P. 41.2%, W. 41.7%—average, 41.0%. In audible presentations: Bi. 43.5%, Bu. 43.5%, A. P. 26.2%, E. P. 42.5%, W. 36.0%—average, 38.3%. Within the mixed series the aural memory is with one exception stronger than the visual, i.e., we have the interesting result, that in the united action of the senses of sight and hearing, their relative strength is just the reverse of what it is when they act independently. *When isolated the visual memory surpasses by far the aural; when combined the aural excels the visual.*

If we disregard the difference of audible and visible modes of presentation, we may consider the contents of our series as: (1) simple contents (series consisting entirely of numbers or of colors); (2) grouped contents (a group of colors followed by a group of numbers or the reverse); (3) alternate contents (a single color followed by a single number); (4) paired contents (a number and a color presented together); (5) doubled contents (a color or a number visible and its name audible at the same time). Beginning with the simple content, the errors are for numbers: Bi. 15.2%, Bu. 16.2%, A. P. 16.5%, E. P. 23.8%, W. 18.1%—average, 18.0%. Colors: Bi. 26.6%, Bu. 27.2%, A. P. 24.1%, E. P. 33.7%, W. 30.6%—average, 28.4%. The memory for numbers was accordingly much stronger than for colors.

With the grouped content there is a greater difficulty than in recollecting the simple content. Error for numbers in the grouped content: Bi. 26.5%, Bu. 34.7%, A. P. 15.7%, E. P. 18.1%, W. 20.4%—average, 23.1%. For colors: Bi. 43.1%, Bu. 36.1%, A. P. 13.8%, E. P. 35.8%, W. 30.4%—average, 31.8%.

With the alternate content the various series yield the following data: For numbers: Bi. 46.1%, Bu. 37.8%, A. P. 21.1%, E. P. 21.7%, W. 32.8%—average, 31.9%. For colors: Bi. 50.6%, Bu. 60.0%, A. P. 39.4%, E. P. 48.9%, W. 38.3%—average 47.4%.

With the paired content the error for numbers is: Bi. 41.2%, Bu. 35.6%, A. P. 18.7%, E. P. 33.1%, W. 39.4%—average, 33.6%. For colors: Bi. 56.8%, Bu. 63.1%, A. P. 55.0%, E. P. 48.1%, W. 53.7%—average, 55.3%.

The general result of these four contents therefore is *that the memory is impeded by a closer combination of different contents.* The more closely numbers and colors are united in presentation, the weaker the memory. For 20 numbers alone or 20

colors alone the average error was 23.2%; for 10 numbers followed by 10 colors or the reverse, 27.4%; for 10 numbers alternating with 10 colors, 39.6%; for 10 pairs of numbers and colors, 44.5%. In all groups the error for numbers is smaller than for colors; this difference increases with the increasing closeness of the two contents.

The doubled content may finally be compared with those series in which the same number of figures or colors was only audible or only visible. The errors are these:—Ten numbers heard: Bi. 6.2%, Bu. 4.3%, A. P. 17.5%, E. P. 23.5%, W. 18.9%—average, 14.1%. Ten numbers seen: Bi. 5.3%, Bu. 11.2%, A. P. 13.7%, E. P. 15.9%, W. 6.3%—average, 10.5%. Ten numbers seen and heard at the same time: Bi. 6.2%, Bu. 2.5%, A. P. 2.5%, E. P. 7.5%, W. 1.2%—average, 3.9%. Ten colors heard: Bi. 25.0%, Bu. 30.3%, A. P. 26.4%, E. P. 32.5%, W. 32.1%—average, 29.3%. Ten colors seen: Bi. 15.3%, Bu. 17.8%, A. P. 13.6%, E. P. 20.9%, W. 21.9%—average, 17.9%. Ten colors seen and heard at the same time: Bi. 5.0%, Bu. 8.7%, A. P. 0.0%, E. P. 7.5%, W. 3.7%—average, 4.9%. *A series of presentations offered to two senses at the same time is much more easily reproduced than if given only to sight or only to hearing.*

We consider finally the differences of simultaneous and successive presentations. As all the audible series had to be successive, the pure visible series only give a basis for comparison. Half of the visible series were successive, the other half simultaneous, both halves corresponding in every respect except in the manner of presentation. The time was the same, as 20 simultaneous presentations were to be looked at during 40 seconds and each of the 20 successive ones for 2 seconds each. The error was for simultaneous series: Bi. 13.6%, Bu. 9.8%, A. P. 15.3%, E. P. 17.9%, W. 15.0%—average, 14.3%. For successive series: Bi. 18.5%, Bu. 23.3%, A. P. 16.6%, E. P. 24.1%, W. 21.6%—average, 20.8%. With each observer the memory was stronger for simultaneous than for successive presentations.

B. The Intensifying Effect of Attention.

(With the assistance of Mr. N. Kozaki.)

It is usually held that when the attention is directed to objects of sense, its effect is not only to increase the clearness and liveliness of the impressions, and strengthen the resulting associations in consciousness, but also to intensify the impressions themselves. The majority of psychologists, influenced partly by the experiences of daily life and partly by theoretical considerations, have acceded to this popular view. But Fechner long ago pointed out that a piece of gray paper does not appear lighter, nor the ticking of a clock louder, however much we direct the attention to them. And more recently Stumpf has shown in his *Tonpsychologie* (I. 71, II. 291) that the ordinary views are by no means self-evident. James (*Princ. of Psychology*, I. 426) comes to the same conclusion as Stumpf, and closes his discussion of the matter with the remark: "The subject is one which would well repay exact experiment." An account of such experiments I shall now give.

The only experiments on the subject (those of Helmholtz and Stumpf) concern the bringing into prominence of one from among several simultaneous impressions. It is clear that the interpretations here may differ. Our problem was to arrange the experiments in such a manner that the intensities of two impressions of moderate strength could be compared, and at the same time the attention be directed toward one and away from the other. In this way we examined intensities produced by light, sound, and the lifting of weights, and also the distances between visible points, the distances serving as measures for the intensity of the sensations produced by the movement of the eyes. The method always employed for diverting the attention was as follows: the subject was directed to give his attention fully to the adding of numbers, which in the case of the optical impressions were read to him, and in the case of the auditory impressions were read by him. The adding took place before and during the time the stimulus was present. Since the order of the stimuli to be compared is of great influence upon the judgment, two sorts

of experiments were arranged for each series. In one case, the attention was directed to the first stimulus, while the second was perceived with diverted attention; and in the other case, the attention was directed to the second stimulus, while the first was perceived with diverted attention. In order to discover from these series the influence of the attention, independently of other conditions, both series must be compared with the results of experiments in which the attention was either directed to both stimuli, or turned away from both. If we designate attention to the first stimulus by A, and that to the second by A', and, correspondingly, the inattention by I and I', we have then for each sense and for the same magnitude experiments with $A—A'$, $A—I'$, $I—A'$, and $I—I'$. $A—A'$, as well as $I—I'$, give the constant error resulting from position, although with a different mean variation. It appears, however, that the results in the two series are different; the overestimating of the second stimulus in the case of $A—A'$ being much more marked than in the case of $I—I'$. Accordingly, we should compare those series only in which the judgment is made under the same conditions. That is, $A—A'$ ought to be compared only with $I—A'$, and in the same way $I—I'$ only with $A—I'$. And from such comparisons the direction of the changing influence of attention must appear. Obviously the actual numbers are valid merely for the relations of these stimuli chosen arbitrarily, and only their relative value, considered as plus or minus, comes in question.

The optical distances were given by an apparatus consisting of a black cloth surface, 80 cm square, upon which were two white points. The vertical distance between these points could be changed by a screw upon the back of the screen, and the exact distance moved could be accurately read (*Beiträge*, Heft IV.). At the beginning of each experiment the white points were covered by a strip of black paper. This was swung to one side, leaving the points visible for 3 sec.; the strip was then returned to its place for 6 sec., during which time the distance of the points was changed; and finally the points were again made visible for 3 sec. The normal distance which was exposed first in one half of the series and

second in the remainder was 30 cm. This was compared with 27.5, 28, 28.5, 29, 29.5, 30, 30.5, 31, 31.5, 32, and 32.5 cm, the various pairs being given in irregular order but symmetrically in each series. The judgment was then noted, i.e., whether the second length appeared greater, equal to, or less than the first. For the light-stimulus a gray was used, produced by a black rotating disk with a white sector. The disk was hidden behind a black screen, then shown for 3 sec.; then it was covered for 8 sec. while the sector was changed, and finally shown again for 3 sec. The normal size of the white sector was 90°, and this was compared with 65°, 70°, 75°, 80°, 85°, 90°, 95°, 100°, 105°, 110°, and 115°. The striking of a metallic ball upon an ebony plate served for the sound. The ball was held by an electro-magnet and fell at the breaking of the current. The time between the two sounds was 5 sec. The normal height of the fall, 50 cm, was compared with 35, 40, 45, 50, 55, 60, and 65 cm. A signal preceded the sound, and simultaneously with this the adding of the numbers began as they were read. The weight was given by lifting a funnel-shaped vessel, held between the thumb and first finger. The elbow rested upon the table, and the weight was raised without movement of the wrist. Weights were put into the funnel in such a way that they could be easily changed. The funnel was supported from below, and the hand lifted it and after 4 sec. lowered it again. After a pause of 6 sec. it was again raised for 4 sec., but with the weight changed. A weight of 300 gm, including, of course, the weight of the vessel, was compared with weights varying from 250 gm to 350 gm with successive intervals of 10 gm.

From only two of the five subjects did we get complete series with weights (1280 experiments with each); from two in the distance experiments (440 with each); from four with the lights (440 with each); and from two with sounds (280 with each). The calculation was made by dividing the judgments into correct and false; the false being those of overestimation (i.e., smaller taken for equal, or smaller and equal for greater), and those of underestimation (i.e., equal taken for smaller, or greater for equal or smaller). Then the per cent was calculated, and the preponderance of the overestimation

over the underestimation (or *vice versa*) was determined. The number of the overestimations, or underestimations, depends upon the sensitiveness and upon the attention at the time at which the judgment is given. The preponderance of the one over the other gives the constant error. The question therefore is, how the constant error changes when calculated first for *A*—*A'* and then for *I*—*A'*, or first for *A*—*I'* and then for *I*—*I'*. The absolute numbers are of course dependent upon the chosen gradations of the stimulus, and consequently have no value of their own. On the other hand, the direction of the change ought to bring out clearly the law of the relation. The results are as follows:

Distances.—First person. *A*—*A'*. The second stimulus was correctly judged, 72.2%; overestimated, 18.8%; underestimated, 9%. Preponderance of overestimation, 9.8%.

I—*A'*. Second stimulus correctly judged, 63.7%; overestimated, 21%; underestimated, 15.3%. Preponderance of overestimation, 5.7%. If *A*—*A'* gives 9.8% and *I*—*A'* only 5.7%, for the preponderance of overestimation, the difference means that the second stimulus is overestimated more frequently by 4.1%, or that the first stimulus is underestimated by that amount, when the attention is turned to the first stimulus. The directing of the attention, therefore, causes the first stimulus to appear weaker.

The same result followed for *I*—*I'* and *A*—*I'*. In the case of *I*—*I'* the second stimulus was correctly estimated 61%; overestimated, 22.5%; underestimated, 16.5%. Preponderance of overestimation, 6%. In the case of *A*—*I'*. correctly judged, 69.4%; overestimated, 21.2%; underestimated, 9.4%. Preponderance of overestimation, 11.8%. The overestimation of the second stimulus, or the underestimation of the first stimulus, occurs therefore more frequently (11.8%, — 6%, i.e., by 5.8%) when the attention is directed to the first stimulus.

Second person. *A*—*A'*, preponderance of overestimation, — 3.2% That is, with this person the underestimation of the second stimulus preponderates. *I*—*A'*, preponderance of overestimation, — 11%. Difference, 7.8%, by which the first

stimulus is less often overestimated, when the attention is turned to it.

I—I', preponderance of overestimation, − 2.4%; *A—I'*, + 3.5%. Difference, 5.9%. For both persons, therefore, the distances, when attentively observed, appear smaller than those perceived in a state of inattention. This is true for both classes of judgments.

Sound.—First person. *A—A'*, preponderance of overestimation, 16.2%; *I—A'*, 9.4%. Again, therefore, the attention causes the first stimulus to appear smaller. *I—I'*, 2.5%; *A—I'*, 5.8; that is, the effect of the attention here also is to weaken the first stimulus.

Second person. *A—A'*, 4.7%; *I—A'*, 4.0%; *I—I'*, 1.6%; *A—I'*, 3.2%. The differences are here exceedingly small, but in each case in the direction that shows the weakening effect of the attention.

Light.—I. *A—A'*, 0.6%; *I—A'*, 5.7%; *I—I'*, 5.5%; *A—I'*, − 2.7%. Here the effect of the attention is an intensifying one in both cases. The same occurs in a much less degree with a second subject.

II. *A—A'*, 3.8%; *I—A'*, 4.4%; *I—I'*, 1.5%; *A—I'*, 1%. But in the case of two other subjects there is again a marked decrease due to the attention.

III. *A—A'*, 16.2%; *I—A'*, 9.7%; *I—I'*, 4.8%; *A—I'*, 8.9%.

IV. *A—A'*, 14.7%; *I—A'*, 10%; *I—I'*, 7.5%; *A—I'*, 12.2%.

Finally, the diminishing effect of the attention comes out most strongly in the experiments with weights.

Weights.—I. *A—A'*, 10.8%; *I—A'*, 2%; *I—I'*, 3.7%; *A—I'*, 8.5%.

II. *A—A'*, 4.3%; *I—A'*, − 7.6%; *I—I'*, 2.7%; *A—I'*, 8.3%.

The unexpected result is reached, therefore, *that all stimuli appear relatively less when the attention is from the outset directed to them.* The light experiments with two persons form the only exception. Now it has often been urged that the changes in the brightness of a gray disk do not always run subjectively parallel to the physical changes. Physically speaking, the increase of darkness is a decrease of light. Psychologically speaking, the increase of darkness may be looked upon as an increase of a positive quality, just as well as

the increase of light. If we grant, now, that these two subjects were inclined to perceive the changes in this sense, the intensity of the darkness is with them lessened by the attention, and thus the single exception vanishes. But even if we disregard these results, those for distances, sounds, and weights remain fully in agreement with each other. The attention causes the intensities to appear smaller. The explanation seems to me to rest in the fact that we must always judge intensities relatively, the standard being in our muscular tensions. The interesting experiments of G. E. Müller have shown how much we underestimate weights if, in consequence of a particular preparatory motor adjustment, we lift them with too great a tension of the muscles. Exactly this ought to occur in cases in which the attention is previously prepared. We judge the intensities relatively to our sensations of tension. Let these, however, be previously strengthened by expectant attention and the stimulus will appear weaker than if the stimulus itself were to arouse reflexly all the corresponding muscular tensions. We interpret our retinal images as spatial magnitudes on the basis of our feelings of convergence and accommodation. In the same way the feeling of intensity comes to represent the intensity of a stimulus only through its relation to the subjective sense of strain. If we purposely strengthen the subjective strain simultaneously with the strains aroused by the stimulus, the stimulus will indeed appear stronger, because we interpret the tension as the result of the stimulus. In the same manner, if we wish, we can make certain beats of a rhythm appear stronger by a voluntary accentuation. If, on the other hand, the tension precede the stimulus as an element in the preparatory adjustment of the attention; and if consequently it be interpreted by consciousness, from the outset, as a subjective function, the increase of the tension aroused by the stimulus can appear only slight, the ratio of the two intensities has become reversed, and accordingly the stimulus is slightly underestimated.

XUM

C. A Psychometric Investigation of the Psycho-physic Law.

(With the assistance of Mr. W. T. Bush.)

The theory of the judging and comparing of differences of sensation has thus far rested chiefly upon the study of just perceptible and just imperceptible differences. But recently the conviction has arisen more and more that as soon as the deductions from such experiments step beyond the mere establishing of a threshold of excitation, they come upon presuppositions, which are at least open to discussion. Sensations are not magnitudes which can be added. Weaker sensations are not contained in stronger. Differences of sensation, then, should appear equal to us only when they arouse in us equal feelings of difference; which feelings depend upon the degree of the similarity of the sensations, and upon other circumstances. I have already tried to show that it is probable that we measure and compare these feelings of difference subjectively by means of sensations of tension. The question arises, how far we also possess objective means for measuring the differences of sensation. The degree of the subjective difference is obviously identical with the relative ease of discrimination. If, then, we possessed a measure for the ease with which we discriminate between two stimuli, we should have a measure of their subjective difference. In time-measurements we do have exactly this. We ought to designate as equal the differences between two pairs of intensities of stimuli, if equal times are necessary to distinguish them. It is clear that this method, which frees us from just perceptible differences, has at the same time the advantage that it is applicable to the study of differences of quality, as well as of differences of intensity. If the psycho-physic law be true, the time necessary to discriminate between weights of 100 and 200 grams must be just as long as the discrimination-time for 200 and 400 grams.

That group of stimuli, with which we began the study of this relation, consisted of lines of various lengths. It is well known that in this field the threshold of excitation follows Weber's law within wide limits. The problem, then,

was to compare stimuli whose differences were always clearly perceptible. To eliminate individual differences, the measurement of the time was effected by the method of chain-reaction, the fundamental principles and advantages of which I have elsewhere described (*Beitr. z. exp. Psych.*, Heft IV. p. 40). A chain of persons so reacted that the first person gave the stimulus to the second, and the second through his reaction gave the stimulus to the third, and so on until the last person gave the stimulus again to the first. The occurrence of the first stimulus and that of the last reaction accomplished respectively the opening and closing of the current in a Hipp's chronoscope, and so the time was measured which elapsed between the first stimulus and the last reaction. If we are dealing with reactions after choice, as in this case, it is clear that the time occupied is the sum of the discrimination-times of all the persons, with the exception of the first. The first person knows the stimulus, and therefore has no choice to make at the end, but only a simple reaction. If we subtract the simple reaction-time of this first person from the total time, and divide the remainder by the number of other persons, we obtain the average discrimination-time, whose relative value must change with the difficulty of the discrimination. For optical stimuli this method is of course not applicable without the use of complicated apparatus. The instruments which we employed permitted the use of five different optical stimuli for any desired number of persons. Each subject had upon the table before him a black metal plate 50 cm square. This was inclined a little away from the subject and had in its centre a round opening 6 cm in diameter. The eyes were opposite this window and at a constant distance from it. At the same time, the five fingers of the right hand rested upon a set of five electric keys, made invisible by a box. If one of these five keys was pressed down by the first person, one of the five pictures appeared immediately in the window of the second instrument. This was accomplished by five electro-magnets, on the back of the plate. If the armature of one of these was drawn down, a lever was released at the end of which there was a circular frame the same size as the window. As soon as this lever was released, it was thrown forward by a strong spring

so that the frame stopped exactly behind the window, and the paper in the frame—upon which might be pictures, or colors, or words, or, as in this case, black horizontal lines upon a white ground—became visible to the subject. The five levers and the five frames (all of hard rubber), and the five electro-magnets, corresponding to the five electric keys, were so symmetrically arranged that every lever had exactly the same distance to move. To obviate the possibility of observing the direction from which the lever came,—which would facilitate the discrimination of the optical stimuli,—the window was covered by a black shutter of hard rubber, which was opened in two halves, through the action of electro-magnets and springs, as soon as one of the keys was pressed down. The same current that carried the frame to the window opened the shutter in front of it, and thus the recognition of direction was rendered impossible. One electric battery was sufficient for the whole apparatus, and it was only for the chronoscope that a second battery was necessary.

We made use of three only of the five frames and worked with six instruments; the total time thus always included the reaction-time of the first subject, Mr. Bush, and the discrimination-time as well as the choice-time of the other five. In the first three frames of each instrument there were—to choose a single example—white disks with lines 5, 10, and 15 mm long. Each subject had upon the table before him a corresponding disk with a line 10 mm long, and was expected to look at this until immediately before the time when the line to be judged came to the window of the instrument. This line was 5, 10, or 15 mm, according to the key which his neighbor had pressed; and a judgment was to be given as quickly as possible as to whether the line were shorter, equal, or longer than the standard line seen immediately before. Then as quickly as possible the corresponding key was to be pressed, the first if shorter, the second if equal, and the third if longer. Since the working of the instrument, as well as the choice between the three fingers and the movements themselves, remained the same in all the experiments, and since all the magnitudes experimented upon were clearly distinguishable, the differences

in time were in direct relation to the differences in discrimination.

To come more closely to the question of the pscho-physic law, we were obliged to choose such intensities of stimulus as were multiples of certain simple differences. For such fundamental differences we chose first, 2.5—5—7.5 mm, secondly, 4—5—6 mm, and thirdly, 4.5—5—5.5 mm. From each of these relations four multiples were investigated; i.e. 4—5—6, 8—10—12, 12—15—18, 16—20—24. In this way we obtained twelve groups of experiments. Each group contained four series, and each series fifteen experiments, which followed one another in a wholly irregular arrangement and included five greater, five equal, and five smaller stimuli. In each of the twelve groups, therefore, there are twenty cases of each kind. And since in every case the choice-times of five persons were measured at the same time, we have 3600 discrimination- and choice-times, which were scattered throughout the entire winter of 1892–93, and were conducted with all possible regard to practice, fatigue, etc.

The averages must be especially calculated from each set of twenty cases; first, because the ratio, 4 : 5, is not the same as 5 : 6, and therefore the discrimination will also be different; and secondly, because the equal judgments must not be confounded with the greater or the less.

If now 0.15 sec. be subtracted, as representing the reaction-time of Bush, and the remainder be divided by five, so that every number represents the average of a hundred single reactions, we have the following results: (σ = 0.001 sec.).

First Group.

	2.5—5—7.5 mm	
Smaller: 512σ	Equal: 574σ	Greater: 530σ
	5—10—15 mm	
Smaller: 491σ	Equal: 551σ	Greater: 527σ
	7.5—15—22.5 mm	
Smaller: 460σ	Equal: 539σ	Greater: 505σ
	10—20—30 mm	
Smaller: 443σ	Equal: 519σ	Greater: 505σ

Second Group.

	4—5—6 mm	
Smaller: 572σ	Equal: 737σ	Greater: 685σ
	8—10—12 mm	
Smaller: 557σ	Equal: 733σ	Greater: 637σ
	12—15—18 mm	
Smaller: 526σ	Equal: 710σ	Greater: 600σ
	16—20—24 mm	
Smaller: 534σ	Equal: 647σ	Greater: 578σ

Third Group.

	4.5—5—5.5 mm	
Smaller: 792σ	Equal: 856σ	Greater: 836σ
	9—10—11 mm	
Smaller: 756σ	Equal: 844σ	Greater: 775σ
	18—20—22 mm	
Smaller: 698σ	Equal: 778σ	Greater: 760σ
	27—30—33 mm	
Smaller: 682σ	Equal: 772σ	Greater: 740σ

All the figures are of course relatively large, since they include the discrimination-time for three magnitudes and the will-time for three fingers. It is especially noticeable that the time in the equal cases is without exception longer than in the greater and smaller cases. This fact, which was also often subjectively noticed, rests evidently upon the consideration that, in the cases of the longer and shorter judgments, there was a summation of two similarly directed judgments. The longest line is longer than the middle one, and much longer than the shortest. This latter judgment strengthens the former, and so the correct reaction is accelerated. In the case of the middle line, however, the two opposite judgments are mutually conflicting. The estimating of the longer line, also, is shown no less regularly to take longer than the estimating of the shorter. The reason lies evidently in the fact that all the relations of the lines represent multiples of three numbers, which have equal absolute differences: and, consequently, the relative difference between the longer and the middle lines

is less in each case than the relative difference between the middle and the shorter lines. To discriminate between 5 and 6 must therefore take longer than to discriminate between 5 and 4. This leads us to the psycho-physic law, which claims that equal subjective differences are correlated with equal objective relations of stimuli. There is no doubt that this law is corroborated for line-lengths by our psychometric investigation. The four series of each group maintained the same relations of stimuli. A wide-reaching constancy in the corresponding numbers cannot be disputed. Group I., containing the relations 2.5—5—7.5, multiplied by one, two, three, and four, gives for the shortest line intermediate values from 443σ to 512σ; for the middle line, from 519σ to 574σ; and for the longer line, from 505σ to 530σ. Group II., containing the relation 4—5—6, multiplied likewise by one, two, three, and four, gives for the smaller line intermediate values from 534σ to 572σ; for the middle line, from 647σ to 737σ; and for the longer line, from 578σ to 685σ. Finally, Group III., with the relation 4.5—5—5.5, multiplied by one, two, four, and six, gives for the shorter line $682—792\sigma$; for the middle line, $772—856\sigma$; and for the longer line, $740—836\sigma$. The limits between which the intermediate values of the three groups vary do not overlap; the figures for the shorter line being, in Group I., $443—512\sigma$; in Group II., $534—572\sigma$; in Group III., $682—792\sigma$; and for the other categories equally marked differences. The approximate validity of the psycho-physic law for optical distances admits of proof, therefore, by psychometric methods.

But at the same time it is evident that this validity is only approximate, and that there exists a perfectly regular variation from the law, in that the subjective difference increases, i.e., the difficulty of the discrimination decreases, with the increasing length of the line. In every group we see that the times become smaller, the higher the number by which the fundamental relation is multiplied. While $792—856—836\sigma$ are the times that correspond to the numbers 4.5—5—5.5 mm, only $682—772—740\sigma$, i.e. about a tenth of a second less, is needed with 27—30—33 mm lines, six times as long. For our subjective discrimination, therefore, *the stronger effect of the*

relative differences of stimuli is constantly influenced by the weaker effect of the absolute differences of stimuli. The numbers show clearly how superior the psychometric method is to the other psycho-physic methods, for the finer analysis of the processes in an act of discrimination; and that too without regard to the fact that we are freed from the necessity of using the just perceptible differences in a way that is theoretically questionable. Similar psychometric investigations with weights, sounds, and lights are in progress, but are not yet completed.

D. Optical Time-content.

(With the assistance of Mr. A. R. T. Wylie.)

The discussion on the time-sense, which has been carried on in recent years with more warmth than politeness, has unfortunately not led thus far to much agreement either as to results or as to explanations. On the other hand agreement has happily been reached in the statement of the question. It is not our task to follow the earlier investigators and pile up numbers about the comparison of times, without regard to the subjective means by which we judge the magnitude of the times; but rather to study these subjective means, and so to arrange the experiments that the analysis of the characteristic psychical factors is made possible. We are also agreed that the results of experiments thus far made are not sufficient for the support of the common theories. They must be supplemented in various directions. Another point is also universally granted: that the judgment of longer time-lengths, as hours, days, years, which rests upon the manifoldness of the presentations, and is indirect throughout, must be clearly distinguished from the comparison of lesser time-lengths, e.g., parts of a minute. In the latter, it is true, the indirect time-consciousness is not wholly wanting; but the direct time-sense stands foremost. Our problem is to study the mechanism of this direct time-sense.

Farther this again is universally admitted, viz., that the judgment of the shortest times, say periods shorter than two seconds, does not take place under the same conditions as the judgment of longer time-lengths, say five, ten, or twenty

seconds. The study of these two processes is accordingly to be kept distinct. The investigation of such longer time-lengths is theoretically the more important, since it allows a wider latitude to the change of outer conditions, as well as to self-observation. With this in mind I prepared the investigations on time-content which I published a short time ago (*Beiträge zur exp. Psych.*, Heft IV. p. 89). The experiments gave the general result, that time-intervals of from eight to twelve seconds, marked off and filled with auditory impressions, can, with the most varied content, be compared with sufficient accuracy to show that the judgment does not depend upon the number of separate presentations in the interval; while on the other hand these presentations do exercise a constant influence upon the estimate of the time. It was shown, namely, that those lengths were constantly underestimated, whose contents highly engrossed the interest of the observer. Words appeared shorter than noises, verses shorter than the strokes of a pendulum, chords shorter than simple tones, and sentences shorter than strings of nonsense-syllables. These results corresponded throughout with those theoretical views which I had formed earlier, on the basis of the self-observation of my subjects, the analysis of the results of others, and experiments of my own. And all the more recent researches substantiate these views much more strongly than the experimenters themselves are inclined to admit. I mention here only one point drawn from these theoretical considerations. The subjective measure for such time-lengths seems to me to lie in sensations peripherally aroused by muscular activity, especially by the strains and relaxations which take place in the various groups of muscles conditioned upon bodily reactions to changing intensities of stimuli. Such reactions occur in the functions of breathing, in the voluntary movements of the eyes, limbs, etc. We can therefore compare intervals with some certainty, even when the number of outer stimuli filling them is quite varied. On the other hand we lose that standard of comparison as soon as our attention is fully directed to those outer stimuli and thus withdrawn from our muscular sensations. This is plain from the fact that at the start all subjects are quite helpless in the comparison of

intervals with different contents. The influence of practice consists just in this,—that one learns to divide the attention between the presentations of the outer stimuli and the bodily sensations. The more strongly these stimuli absorb the attention, the more must the bodily sensations retreat into the background of consciousness, even with experienced observers, and the shorter must the lapse of time appear. The less interesting the stimuli, the more obtrusive the bodily sensations and the longer the apparent time. It is clear that the results mentioned correspond perfectly with these theoretical deductions.

I then proposed to continue the research by investigating the influence of the time-content, when the marking off and filling of the time-intervals was accomplished by optical in place of auditory stimuli. Such optical experiments upon the time-sense have thus far never been made, if we disregard earlier investigations on the comparison of rapidities.

The method of investigation was as follows: a Ludwig kymograph, with drum placed vertically, was so set that every point moved exactly one centimeter in a second. Between the drum and the subjects there was a screen of black paper, in which an opening 2 cm square was cut. Immediately behind this opening moved the black paper which covered the drum. On a level with the opening there were fastened pieces of paper of any desired color or degree of brightness, of uniform surface or covered with print. These passed by the square opening at the rate of one centimeter a second, and the lengths to be compared followed one another immediately with no intervening period. Suppose for example that yellow and red papers are fastened to the drum. The subject sees first a black, then a yellow, then a red, and finally again a black square, and judges whether the red seems to occupy a longer, shorter, or equal time, as compared with the yellow, in passing by the opening in the screen. If letters or numbers were employed as stimuli, the white papers on which they were printed served as the lengths for comparison. The numbers and letters were arranged singly, or in groups, at such irregular intervals that their number gave the subject no clue whatever. The

question is really therefore one of comparing spatial extents, in which, since the lengths pass by with equal rapidity, our time-sense can give the only standard for judgment. The lengths to be compared were so chosen that in every experiment one length was 10 cm, and the other 7, 8, 9, 10, 11, 12, or 13 cm. Every full series comprised twenty experiments, in which 10 was compared four times with 9, 10, and 11 respectively, and twice with 7, 8, 12, and 13. And, further, in ten cases 10 cm served for the first, and in ten cases for the second length. These twenty possibilities followed irregularly in every series. Then in the next series the order of the first series was reversed. If at the start yellow was the first and blue the second length, in the succeeding twenty experiments blue was the first and yellow the second. Then the same forty experiments were repeated. The experiments were, therefore, distributed with perfect symmetry, exactly as in the case of my auditory experiments, which have been already described. Meumann's criticism of my method has only the more strongly convinced me of its correctness.

Obviously the time-order exercises a great influence. A strong tendency was shown to overestimate the second length. Since, however, the influence of the position does not here concern us, we have only to eliminate it by reckoning together the results of the symmetrical experiments in both positions. If in one hundred experiments yellow was first and blue second, and in another hundred blue was first and yellow second, we have to ask how often in two hundred experiments the yellow appeared longer than the blue. We find that the one content was objectively longer than the other forty times in every one hundred experiments, equal to it twenty times, and shorter forty times. If we halve the equal judgments and assign them equally to both sides, fifty per cent would constantly appear longer and fifty per cent shorter—if the quality of the content were without influence—since the influence of position has been eliminated. The actual result may now be given.

Six subjects took part, and each one was given more or less preliminary practice; for here as in other investigations some

practice proved necessary. The good experiments were as follows:

I. Yellow and light green were compared. The results fluctuate throughout and show no decided tendency. Manifestly both colors make equal claims upon the attention.

II. Brilliant yellow and dark red. A decided tendency in each person is clearly manifest, but it varies with the individuals. In one hundred cases yellow is estimated longer, as follows: with B., 62.3%; H., 67.8%; J., 42.6%; K., 46%; P., 43.8%; W., 39.5%; that is, two persons overestimated and four underestimated. The tendency is in every case so strong that there may be a question here as to the individually different attitudes of the attention. In all the following groups, however, the tendency is perfectly uniform.

III. A continuous color is compared with a series of from six to ten strips of different colors and of varying widths. The many-colored band appears regularly shorter than the continuous color. In each one hundred experiments the one continuous color appeared of longer duration: for B., 59.5%; H., 63.8%; J., 51.2%; K., 54%; P., 65.3%; W., 58.7%—average, 58.7%. Here it appears to me unquestionable that the changing colors attract the attention more than the monotonous color.

IV. A continuous color is compared with a series of from six to fifteen letters on a white background. The letters appear shorter. The color is held to be of longer duration, as follows: with B., 51.2%; H., 55%; J., 49.1%; K., 52.3%; P., 50.8%; W., 62.3%—average 53.5%. The underestimating of the letters is therefore somewhat less. Evidently, single letters, moving by so slowly, have far less tendency to catch the attention than bright changing colors. But the result gains especial interest from comparison with the following group.

V. The same optical impressions as in group IV. But the subjects were obliged now to take care to keep the letters in memory, and write them down after each experiment. The result is that the color appears longer than the letters, as follows: with B., 62.3%; H., 71.5%; J., 53.2%; K., 56.3%; P., 58%; W., 54.9%—average, 59.4%. The more marked straining of the attention leads, therefore, at once to a more marked under-

estimation of the time. This reaches its culmination in the next group.

VI. A continuous color is compared with a series of numbers between one and forty—these to be added as they pass by, and the sum noted. The color appears longer: for B., 75.6%; H., 54.3%; J., 52.8%; K., 69.3%; P., 56.2%; W., 68.7%—average, 62.8%.

With the use of optical stimuli I arrive, therefore, at exactly the same result as with auditory stimuli, viz., *that, irrespective of the number of the presentations, the times appear shorter the more the given optical time-content attracts the attention, and thus diverts it from the observation of the accompanying subjective phenomena produced by bodily changes.*

I limit myself here to the experimental results. What the subjects have said about their subjective experiences in reference to subjective rhythmical strains, respiration, and eye-movements I shall give at another time in a theoretical discussion of the time-sense.

E. A Stereoscope without Mirrors or Prisms.

(With Plate I.)

The stereoscope, as every one knows, is an instrument by means of which the two eyes are made to see different pictures, and yet to converge at a point corresponding to the apparent distance of the object seen. If these two pictures correspond to the views that a person's right and left eyes have respectively of a solid object, they unite, and the two flat pictures fuse into one solid figure. Up to the present, the problem of the stereoscope has been solved either by the use of mirrors (Wheatstone, Helmholtz, Duboscq, etc.), or prisms (Brewster, etc.). Without mirrors or prisms the converging eyes would be obliged to see the same picture always.

But this is a fact for simultaneous impressions only, not for successive impressions; which, as far as I know, have never been considered available for stereoscopic purposes. As the stroboscopic disks and the zoötrope demonstrate, the subjective visual impression considerably outlasts its objective stimulus. If, then, we suppose that for the stereoscopic union it is not the simultaneity of the visual stimuli that is necessary,

but only the simultaneity of the two visual impressions, the effect must also be produced when the two pictures are presented to the two eyes in rapidly-changing succession, in such a way that the impression upon the left eye is still effective when the right eye is stimulated, and *vice versa*. But in this case it is evident that no mirror or prism is necessary, since now the eyes can rest in their natural position of convergence; and the two pictures appear successively at the same spot—provided only that care be taken that each picture be always accessible to the corresponding eye, and to that eye only.

The easiest way of accomplishing this is by using rotating disks. Stroboscopic disks preceded the ordinary stroboscopic drum of to-day; and disks, with slits for the right and left eyes respectively, and separated from one another by equal angles, have also been made use of for other optical purposes (Sanford, Dvorak). The only thing which I found necessary, then, was to fasten such a disk upon the same axis with, and at a certain distance from, another disk, so that the pictures were displayed alternately to the right and left eyes, the pictures being placed at angular distances corresponding to the slits, and at equal radial distances from the middle point. The slits must be as narrow as possible, in order that the pictures may be seen unmoved; and must be as numerous as possible, in order that the pictures may be seen clearly. A trial gave the expected effect with surprising vividness.

The form of apparatus used by me is as follows (see Plate I.): a strongly built color-mixer, or a centrifugal machine placed vertically, is provided with a steel axle 40 cm long, in such a way that one half of the axle is in front and the other half behind the machine. On both the front and the back of the axle is a brass screw, which holds securely a card-board disk. In order that the distance of the disk could be changed at will, I used perforated brass disks with collar-attachments which can be slid along the rod and secured at any desired point. The disk to be fastened in front is made of heavy black card-board, and has a radius of 25 cm. The disk has twelve slits, six inner and six outer. The inner ends of the former are 10 cm from the centre and the slits are 5 cm long: i.e., the outer ends are

15 cm from the centre. At 18 cm from the centre the outer slits begin and they end at 23 cm. The inner and outer slits alternate regularly at angular distances of 30°. The slits are bounded by radii, so that their widths increase from within outwards. The outer slits are 8 mm wide at their outer ends, the inner slits 5 mm. If, now, one sits before the disk in such a way that the eyes are on a level with the shaft, one eye is always obscured while the other sees through one of the slits. If one sits in front of the left half of the disk, the inner slits correspond to the right and the outer slits to the left eye. And if the propelling wheel be set in motion, so that the disk makes, say, five revolutions in a second, each eye must look thirty times a second upon the second disk behind. This second disk, which is fastened to the rear half of the shaft, is, in its simplest form, a circle of white card-board, of 25 cm radius, upon which, at intervals of 30°, are drawn twelve figures whose middle points are 16.5 cm from the centre of the disk. Six of these twelve figures are for the right and six for the left eye, the two kinds alternating regularly. One disk has, for example, the well-known outline of a truncated pyramid, with which the result is that the smaller inner square is six times on the right side and six times on the left side of the larger square. On the other hand, the middle points of all the twelve larger squares are at exactly equal distances from the edge of the disk, so that during rotation they completely cover one another. If now the rear disk be so secured that any one picture for the right eye stands opposite any inner slit, the pictures for the left eye are then, of course, opposite the outer slits. If the shaft be now set in rotation, the eyes can converge readily upon the second disk, which is perhaps 20 cm distant, and then the right and the left eyes look upon the same spot of the background of the rapidly changing pictures. Subjectively, however, we believe that we are continually observing the solid object. The pyramid stands out in relief, as with the best stereoscopes. If one looks in turn through the right side of the disk, one sees the pyramid hollow; since now the outer slits correspond to the right eye, and the right eye, therefore, sees the picture intended for the left eye. For the same reason the succeeding and preceding pyramids upon the left

side also appear hollow, while only that one which stands exactly opposite comes out in relief. Of course pictures for this disk-stereoscope can be printed just as simply as for the zoötrope, etc. Instead of changing the disks each time, we use another variety of disk, which is so provided with slits that the desired pictures can be easily inserted. Instead of the twelve pictures, it is more convenient to make use of two only, as in the case of the stereoscope. The disk with the twelve slits may also be used for the two pictures, if the rear disk be rotated exactly six times as rapidly as the one in front; an arrangement which is technically inconvenient. On the other hand, the twelve slits can be reduced in number, if only the pictures give a sufficiently strong impression. Eight slits with eight pictures generally give strong effects, but two slits with two pictures separated by 180° prove successful only with such clear impressions as, say, white stereometric drawings upon a black ground.

Obviously other forms of movement can also be chosen in order to present the two pictures successively; for example, both pictures may be glued back to back and turned about a horizontal or vertical axis. It depends only upon whether a rotation of 180° takes place between the view of the left eye and that of the right. A very convenient further arrangement is this: a black paper cylinder, of say 20 cm diameter and 15 cm in height, is fastened to a vertical wooden disk and rotated like a color disk upon a centrifugal machine. If two slits are made in this for the two eyes, something like 120° from one another, and the pictures fastened to the inner side of the cylinder opposite the slits and equally distant from the base of the cylinder, so that they cover one another at rotation, the stereoscopic effect is well produced. The slits ought not to be 180° apart, since the pictures would then partly cover up the opposite slits. So it is evident that unlimited variations of the same principle are possible.

Among the practical advantages of this stereoscope I may mention as most important that it admits of an immediate union of the stereoscopic with the zoötropic effects. A dozen figures or more can be easily printed upon a disk just as they are now printed upon a single strip for the zoötrope;

and in such a way that the right eye's views of the phases of movement alternate with those of the left. The effect then is that the solid object is seen in movement. For the illustration of physical apparatus and machines in action, of animals in motion, of physiological and pathological forms of movement in man, and so on, unusually clear representations may be secured. A further advantage is the unlimited size of the stereoscopic pictures to be combined, and the circumstance that for each eye the distance of the picture from it can be chosen at will. Since the rotating wheel for mixing colors is very common in schools and elsewhere, it would require but a longer axle and some paper disks to add to the pedagogical equipment a stereoscope both easy to understand and easy to operate, having also the advantage that it is at the same time stroboscopic and strobo-stereoscopic. But the application of this simple apparatus to theoretical studies appears to me essentially more important. All the questions not only of stereoscopic vision, but also of binocular vision in general, the question of the rivalry of the visual fields, the questions of binocular color-mixing, of contrasts, of lustre, etc., can be studied here from a new point of view. By being able not only to give different pictures to the two eyes, but also to give at the same time different stimuli to each eye successively; by being able to vary at will the time between the stimulation of the two eyes; by being able to present a picture more frequently to one eye than to the other, etc., there arise a multitude of new and interesting problems.

SHORTER CONTRIBUTIONS.

ARITHMETIC BY SMELL.

BY FRANCIS GALTON, F.R.S.,
London.

It seems worth while to put a few simple experiments on record, which I made for my own satisfaction a few months ago, in order to assure myself that arithmetic may be performed by the sole medium of imaginary smells, just as by imaginary figures or sounds. I had first to familiarize myself with a variety of scents, for which purpose the following arrangement was provided. Each scent was poured profusely upon cotton wool, loosely packed in a brass tube $\frac{3}{4}$ inch in outside diameter, which had a nozzle at one of its ends. The other wide-open end of the brass tube was pushed into a tightly fitting piece of caoutchouc tubing, $4\frac{1}{2}$ inches long, and the opposite end of the tubing was stopped with a cork. Whenever the tubing is grasped by the hand, a whiff of scented air is forced through the nozzle; when the grasp is relaxed, fresh air enters through the nozzle and passing through the wool becomes quickly impregnated with scent. The apparatus is then ready to be used again. Whiffs of scented air may thus be sent out four or five times in moderately quick succession and be almost equally odorous throughout. In using the apparatus, I begin by breathing out slowly through the nose, to prevent any scent from being prematurely perceived; in the mean time the nozzle is brought below the nostrils. Then I simultaneously give a sudden grasp and a sudden sniff up. A separate apparatus is used for each scent. They are made as alike as possible, and are scarcely

distinguishable; nevertheless it is well to operate with the eyes shut. The scents chiefly used were peppermint, camphor, carbolic acid, ammonia, and aniseed. I taught myself to associate two whiffs of peppermint with one whiff of camphor; three of peppermint with one of carbolic acid, and so on. Next, I practised at some small sums in addition; at first with the scents themselves, and afterwards altogether with the imagination of them. There was not the slightest difficulty in banishing all visual and auditory images from the mind, leaving nothing in the consciousness besides real or imaginary scents. In this way, without, it is true, becoming very apt at the process, I convinced myself of the possibility of doing sums in simple addition with considerable speed and accuracy solely by means of imaginary scents. Further than this I did not go, so far as addition was concerned. It seemed a serious waste of time to continue the experiments further, because their difficulty and complexity rapidly increased. There were also provoking lapses of memory. For instance, at the present moment, having discontinued the experiments for three months, I find my old lessons almost wholly forgotten. Few persons appreciate the severity of the task imposed on children in making them learn the simple multiplication table, with its 81 pairs of values each associated with a third value. No wonder that they puzzle over it for months, notwithstanding the remarkable receptivity of their fresh brains. I did not attempt multiplication by smell.

Subtraction succeeded as well as addition. I did not go so far as to associate separate scents with the attitudes of mind severally appropriate to subtraction and addition, but determined by my ordinary mental processes which attitude to assume, before isolating myself in the world of scents.

Few experiments were made with taste. Salt, sugar, citric acid, and quinine seemed suitable for the purpose, and there appeared to be little difficulty in carrying on the experiments to a sufficient extent to show that arithmetic by taste was as feasible as arithmetic by smell.

THE PSYCHOLOGY OF INFANT LANGUAGE.

BY PROFESSOR JOHN DEWEY,

University of Michigan.

In his interesting and valuable article on *The Language of Childhood*,* Mr. Tracy undertakes, upon a basis of 5400 words used by at least twenty different children, to determine the relative frequency of the various parts of speech. Before making some remarks, I wish first to submit my own mite for the further use of students. A refers to a boy; B to a girl, 20 months younger.†

A at 19 *mos. old.*

Parts of Speech.		Per cent.
Nouns . . .	68	60
Verbs . . .	24	21
Adjectives .	13	11
Adverbs . .	4	3
Interjections	6	5
Total . .	115	100

Pronouns, prepositions, conjunctions, none.

B‡ *at* 18 *mos. old.*

Parts of Speech.		Per cent.
Nouns . . .	76	53
Verbs . . .	40	28
Adjectives .	2	1
Adverbs . .	9	6
Interjections	7	5
Pronouns .	8	6
Conjunctions	2	1
Total . .	144	100

Prepositions, none.

For purposes of comparison, I append the per cents reached by Mr. Tracy by averaging all his results:

Nouns	60
Verbs	20
Adjectives	9
Adverbs	5
Pronouns	2
Prepositions	2
Interjections	1.7
Conjunctions	0.3
	100.0

* Am. Jour. Psychol., vol. VI., No. 1, reprinted in *The Psychology of Childhood*, Boston, Heath & Co., 1893.

† The presence of other children in the family should always, I think, form part of the data with reference to a child's vocabulary. At least, it is one of the old wives' saws on this matter that the presence of other children both hastens and extends a vocabulary.

‡ A's vocabulary was kept continuously; B's vocabulary was taken from words actually used within a period of five or six days; a number of words contained in her vocabulary four months previously do not appear at all.

I wish to remark (1) concerning the relative frequency of verbs, and (2) concerning the different rates of distribution in different children:

1. Mr. Tracy notes that since the relative frequency of verbs in the language is but 11 per cent, the child, *comparatively* speaking, uses verbs with 1.81 the ease with which he uses nouns, and makes some judicious remarks concerning the prevalence of concepts of activity in the child mind. I think he could make his case much stronger. Mr. Tracy, I take it, has classified his words according to the sense which they have to an adult, and I have followed that principle in my own table.* In a sense, however, this is as artificial as Mr. Tracy notes that it is to put knife under *k* instead of under *n*, because *we spell* it with a *k*. The psychological classification is to class the word according to what it means to a child, not to an adult with his grammatical forms all differentiated.

Such a classification would in all probability increase immensely the percentage of verbs. It is true that such a method demands much more care in observation, and opens the way to the very variable error of interpretation; but the greater certainty of the method followed above is after all only seeming—it does not express the *child's* vocabulary, but our interpretation of it according to a fixed but highly conventional standard. It is out of the question to redistribute the language of A and B, given above; but I subjoin the vocabulary of a child in his twelfth month where contemporaneous observation makes me reasonably sure of what the child means:

See there; bye-bye; bottle; papa; mamma; grandma; Freddy; burn; fall; water; down; door; no, no; stop; thank you; boo (peek-a-boo); daw (used when he sees anything which he wants given to him)—17 in all.

Of the above, only the four proper nouns are, psychologically speaking, names of objects. Water is a verb as well as

* Phrases like 'all light,' 'all dark,' 'all gone,' 'out' (for 'go out'), etc., I have treated as verbs. It is obvious that they might be considered either as interjections or as adjectives. The relatively larger per cent of verbs in my table may be due to this classification.

a noun; door is *always* accompanied by gestures of reaching, and an attempt to swing the door back and fro; 'daw' is apparently a request, an expression of expectation of something good to eat and the name of a thing all together; bottle certainly has adjectival and verbal implications as well as nominal. At present I should regard it as a complex, 'nominal-adjectival-verbal,' the emphasis being on the noun, while six weeks previously it was, say, 'verbal-adjectival-nominal.' 'Stop'; 'no, no'; 'burn'; 'see there', etc., are equally interjections and verbs. 'Thank you' is at times a request for something, and is almost invariably said when giving an article to any one else. We have then a graded and continuous series, so far as *sense* is concerned, the proper names (23 per cent) at one end, and the interjectional forms 'no, no', 'peek-a-boo', at the other. These have a verbal coloring, however. Between these classes are a nominal-adjectival-verbal-interjectional complex, the verbal-interjectional meaning prevailing on the whole, the adjectival in all cases subordinate.* The tendency to apply the same term to a large number of objects ('ball' to ball, orange, moon, lamp-globe, etc.) can be understood, I think, only if we keep in mind the extent to which the formal noun, 'ball,' has really an active sense. 'Ball' is 'to throw' just as much as it is the round thing. I do not believe that the child either confuses the moon with his ball, or abstracts the roundness of it; the roundness suggests to him something which he has thrown, so that the moon is something to throw—if he could only get hold of it.

What I would suggest, then, along the line of a study of the distribution of vocabulary into parts of speech is such observation and record as would note carefully the original sense to the child of his words, and the gradual *differentiation*

* The fact that interjections fail so late, as a rule, in aphasia, taken with the highly immediate and emotional character of child-life, indicates the defective character of a method of classification which reduces the percentage of interjections to 1.7. The philologist's objections to making interjections a primitive form of speech, however sound grammatically, seem to me to rest upon attaching a limited, technical sense to the concept *interjection*, which is without ground psychologically. In the infant mind (whether race or child) the emotional state and the tendency to react aroused by an object *must*, I should say, be fused, and both precede any clear recognition of the 'object' as such, or of any objective quality.

of the original protoplasmic verbal-nominal-interjectional form (as it seems to me), until words assume their present rigidity.

2. No one can examine the statistics given without being struck by the great differences in different children. F, in Mr. Tracy's tables, has 15 per cent interjections; while K, with a vocabulary of 250 words, has none at all. F has 11 per cent adverbs; while K has but 2 per cent; in my own table, A has 4, while B has 9 per cent. So in my two, A has 11 per cent adjectives; B, 1 per cent; while Mr. Tracy's vary from a maximum of 13 to a minimum of 3 per cent. I believe the tendency in all psychological investigation, at present, is to attempt to get a *uniform* mathematical statement, eliminating individual differences; for pedagogical and ethical purposes, at least, it is these differences which are, finally, most important. And on strictly psychological grounds the varying ratio of adverbs and pronouns on one side and nouns and adjectives on the other must denote a very different psychological attitude—different methods of attaching interest and distributing attention. Observation of different mental traits as connected with these linguistic differences would not only add to the *terra incognita*, individual psychology (and it would seem that all psychology must be finally individual), but throw great light upon the psychology of language. How vague and formal at present our answers, for example, when we are asked to what psychological state and need an adverb corresponds!

WORK AT THE YALE LABORATORY.

BY E. W. SCRIPTURE.

The first year in the life of a laboratory is one of incredible difficulties and incessant labor in getting matters arranged. Nevertheless, we have been able to carry on several investigations and bring them to successful conclusion and publication.*

The most extensive investigation was that by C. B. Bliss on reaction-time and attention. The graphic method was developed so that records absolutely accurate to thousandths of

* *Studies from the Yale Psychological Laboratory*, 1892–1893, edited by E. W. Scripture, New Haven, 1893.

a second can be made and counted with less trouble than chronoscope records. The vibrating line receives a spark-record at the instant of the stimulus and another at the instant of reaction. The single waves, .01 sec., are counted and the odd tenths obtained by the eye. The necessary arrangement of the currents led to the invention of a multiple key. The reaction-time to sound was measured with and without disturbances of attention. When the attention was distracted by a steady light the disturbance of the reaction-time to sound was very small; with an unsteady, moving light it was very great. When the attention was disturbed by a steady sound, e.g., a tone, no disturbance resulted in the reaction to sound; with an intermittent sound, e.g., a metronome, it was very marked. In making these experiments some unexpected results were obtained, showing that the reaction-time to a sound heard in both ears is shorter than when the sound is heard only in one ear, even after making allowance for the difference in intensity.

Dr. Bliss made the attempt to determine the relation of changes in reaction-time to various other mental disturbances. After each set of experiments records were made of anything worth noting that had passed in the mind. The conclusions from these introspective observations are:

1. Reaction-time is constantly affected by irregular disturbances a large part of which may be detected by introspection.

2. Introspection is not to be trusted in estimating results.

3. Reactions to the wrong signal, reactions before the signal is heard, and the reflex nature of reactions are not sufficient criteria to distinguish muscular from sensorial reactions.

4. There are at least six distinct kinds of voluntary attention: ideational attention, neural attention, feeling attention, muscular attention, preparatory attention, and inattention.

5. The involuntary attention is constantly changing.

Experiments were made showing the influence of various distractions and mental operations on the rate of voluntary tapping and on the steadiness with which a lever could be kept at a given place.

C. E. Seashore succeeded in making the first trustworthy measurements on the time required for altering the accommodation of the eye. He has established three important principles:

1. Within certain limits the accommodation-time varies with the distance between the points for which the eye is to be accommodated.

2. It takes longer to change the accommodation from near to far than from far to near, and this difference in time varies directly with the length of the accommodation-time.

3. For equal distances in the same range the accommodation-time is greatest for points near the eye and decreases with the distance of the points from the eye.

The investigations on reaction-time in relation to intensity and pitch, made by Dr. M. D. Slattery, lead to these conclusions:

1. The law that the reaction-time decreases with increasing intensity of stimulus does not hold good for the sense of hearing, i.e., the reaction-time to tones is nearly the same for all moderate intensities.

2. The longer time registered for very weak tones or noises by some observers is probably not due to any conscious change, but is caused by hesitation as to the actual hearing of the stimulus.

3. The reaction-time to tones decreases as the pitch rises.

4. The view held by Exner, von Kries, and Auerbach and rejected by Martius—namely, that about ten vibrations are necessary to the perception of a tone, no matter what its pitch —is sufficient to explain the differences in the reaction-times for different tones.

5. In the domain of tactile stimulation by electricity the reaction-time decreases with the increase in the intensity of the stimulus.

The experiments of J. A. Gilbert on the musical sensitiveness of school-children involved in the first place the invention of a new piece of apparatus, the tone-tester, which has proved an exceedingly convenient instrument for much demonstrational work on the psychological methods. The sensitiveness to differences in pitch increases with age; at first rapidly, then

very slowly. The least perceptible difference at 6 years is $\frac{1}{3}\frac{3}{3}$ of a tone, at 19 years it is $\frac{2}{3}\frac{2}{3}$. At 10 years and at 15 years the sensitiveness suddenly falls off.

A new reaction-key, designed to avoid the objections to the usual telegraph-key, is described in an article on the time of voluntary movement. Being a new instrument it opened up new methods of solution; one of these was the measurement of the time of voluntary movement. The time of flexion of the finger was found to decrease as the distance of movement increased from 5 mm to 20 mm; the time of extension, however, increased. This is explained by J. M. Moore as the result of the favorable leverage for the flexor muscles at the smaller distances.

An article on drawing a straight line makes the attempt to apply experimental methods to pedagogical problems. The average errors for various positions, inclinations, grasps of the pencil, etc., were determined.

The equipment of the workshop in the laboratory has proved an excellent investment. The room is the one most used in the building and is often quite overcrowded. To have accomplished without a workshop the amount of work actually done in the laboratory would have cost more than the two hundred dollars spent in its equipment; thus at the beginning of the second year we are in possession of a well-equipped workshop which has already paid for itself. The employment of a mechanic has furnished the opportunity for the invention of several pieces of apparatus. The multiple key has been much improved in a later model. A novel clock contact makes platinum contact in the middle of the arc of swing. A new drum built with the durability of a piece of machinery is run either by hand or by motor.

DISCUSSION.

PROFESSOR WUNDT AND FEELINGS OF INNERVATION.

A note to page 432 of Vol. I. of the fourth edition of Wundt's *Physiologische Psychologie* quotes an opinion of mine and corrects it in a manner that seems to demand a word of reply. When the external rectus-muscle of a man's eye (say the left eye) is wholly or partly paralyzed, objects lying in the left half of the field of view appear to that left eye to lie farther to the left than they really are. In Prof. Wundt's earlier writings he agreed with Von Graefe and others in explaining this phenomenon by the man's consciousness of the excessive leftward innervation which he must employ in turning his diseased eye towards the object. The existence of feelings of innervation was attacked presently by Bastian, Ferrier, and others, and this particular supposed case of it was explained away by G. E. Müller and myself. We pointed out that the true cause of the object's false leftward location was rather to be found in the inward squint of the right eye when the left one vainly or successfully turns to look at the object. The leftward innervation is indeed increased, but there is no need of assuming the *feeling* of it to be increased, when the feeling of its *results* in the turning of the *right* eyeball (even when its lid is closed or it is screened from the object) explains sufficiently why the man should think himself looking farther to the left with both eyes than he really is.

Professor Wundt, in the third edition of his book, definitively abandoned the theory of feelings of innervation. In the present fourth edition he adds to what he has to say upon the subject some novel remarks of detail. *Inter alia* he says that it is impossible to explain the false location of the object in the case before us by the position of the sound, or right, eye. It seems to me, however, that he has failed to understand correctly the facts and his authorities for them, and that Müller's and my explanation stands as firm as it did before.

Wundt says (p. 424): "To the movement of the sound eye the false localization cannot be ascribed, for the images seen by the two eyes are distinct, and only that of the lame eve is falsely placed." In the

note to p. 432 he quotes a passage from Alfred Graefe and thereupon makes the remark that more particularly provokes the present note from me. I must leave Graefe's passage in the transparency and elegance of the original: "*Die Richtung in welcher sich das (dem, paralytischen Auge angehörende) Scheinbild von dem (vom normalen Auge herrührenden) wahren Bilde entfernt, liegt stets in der nach aussen projicirten Wirkungsbahn des gelähmten Muskels, d.h. in der Ebene, welche die Sehlinie um die Drehungsaxe desselben beschreibt.*" Wundt's remark hereupon is: "When therefore W. James (Psychology, ii. 506) and others aver that the displacement of the false image comes from the movement of the normal eye, they would seem to ascribe to this latter the marvellous capacity of a simultaneous twofold localization, first a normal one coming from the said eye's real position, and second an abnormal one, corresponding to the position which the paralytic eye is striving to reach."

What meaning the special quotation from Graefe may have for Prof. Wundt's mind I cannot tell, but the rest of Graefe's text, and the facts themselves are so simple that one wonders how there can be two opinions about them. The case Wundt is considering is apparently that in which both eyes are open, the object lies or moves towards the left, and the sound right eye turns to it and sees it where it is, whilst the lame left eye fails to rotate so as to fixate it and consequently gets its image on the nasal half of the retina or, in other words, sees the object in indirect vision to the left of the point at which it directly looks. During all this there is a convergent squint, the right eye being turned in and looking farther to the left than does the left eye.* The question now is: *where* do the two images appear? The left eye's image must in any case appear to the left of the right eye's image, because whilst the latter falls on the right fovea, the former falls on the nasal half of the left retina. But where does the right eye's image appear? In its real place, or thereabouts, according to all accounts. *Thus the position of the right eye is what determines a place, to the left of which the left eye's image is falsely referred.* There is no question of any twofold localization here by the right or normal eye. That eye sees in the direction of its own line of sight, of which direction it would appear to be made conscious by its feelings of rotation. The left eye also has feelings of rotation, but

* For simplicity's sake I omit the variation in which the left eye succeeds in rotating so as to fixate the object, whilst the right eye turns violently in, and, fixating a point leftward of the object, gets the image of the latter on the nasal half of its retina, or sees it in indirect vision to the right of the spot which it fixates. The principles of explanation are here the same.

they would appear to be overpowered by those of the right eye, first because the actual rotations of the latter eye are the stronger, and second because (as a host of similar pathological examples show) we are liable (until trained by contrary experience) to suppose, when we have intended a movement, that the movement has taken place. The patient intends to move both his eyes considerably to the left. He does so move his right eye only; but failing, in the novelty of the whole experience, to discriminate in his orbital feelings just what new and strange things have occurred, he thinks he has performed the entire movement as usual in spite of the fact that he has not. He sees double; he locates the left eye's image according to the fatal laws of retinal projection; and he gets a strong vertigo as the result of the unusual behavior of the field of view. How Professor Wundt himself would explain the wrong localization by the left eye without invoking either the right eye's position or the feelings of innervation in which he formerly believed, he does not deign to say.

The point is a minute one, certainly in itself not worthy of notice; and the existence or non-existence of feelings of innervation is an alternative on which, so far as I can see at present, no general theoretic consequences seem to hinge. I should consequently not have been stirred to write this note were it not that Professor Wundt's peculiar manner of revising his opinions is objectionable from the point of view of literary ethics, and is beginning, I fancy, to arouse in other readers besides myself an irritation to which it is but just that some expression should be given.

First, it would seem better, in issuing revised editions of works as weighty as those of this author, to name explicitly in the new prefaces the pages where modifications of doctrine are to be found. No one ought to be forced to read a thousand pages merely to ascertain what an author's newest formulations are. Second, it would seem well, in parts of the text where a change of view has occurred, to announce that fact explicitly in the text. And third, it would be fair, if one cited authors already identified with the new view, to cite them so as to award to them some degree of credit. In this overburdened age the reader has a right to clearness on every point. But Prof. Wundt's new prefaces contain no reference by pages to what is revised; his text habitually lacks any indication that his thought may once have been different from what it is; and his citations are almost always by way of discrediting the predecessors quoted and clearing their opinions out of the way. No one, I think, who should be introduced to Psychology by Wundt's third edition could come to any other conclusion than that Bastian, Ferrier, and others were adherents of a

foolish theory of innervation-feelings to which Wundt himself now and ever stood opposed. In the fourth edition Münsterberg, one of the most original opponents of innervation-feelings, is quoted only once and then actually so as to make the reader think that he might most naturally have got his views from Wundt himself (see p. 431, note).

The mania for a plausible smoothness, the shrinking from an appearance of fallibility, seem in fact in Wundt's later writings to be driven so far as seriously to neutralize the clearness and value of the work. A thinker so learned, so intelligent, before whose encyclopædic capacity an entire generation bows down with cordial admiration, ought to be above such foibles. Not in such ways were the best parts of the reputation of a Fechner, a Mill, a Darwin, made.

WILLIAM JAMES.

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MR. JAMES WARD ON MODERN PSYCHOLOGY.

Mr. Ward's noteworthy article in *Mind* for January last, under the title '*Modern' Psychology: a Reflexion*, may be described as a critique of the fundamental conceptions on which Prof. Münsterberg and others of the younger physiological psychologists base their experimental work. It was in his brilliant little book, *Die Willenshandlung* (1888), that Prof. Münsterberg first advanced the view that all mental states, emotions and volitions as well as cognitive states, are simply complexes of sensations, that is, of elements each essentially similar to blue, hot, sour.* This view was briefly but severely criticised by Prof. Wundt in his article 'Zur Lehre von den Gemüthsbewegungen' (1890), who designated it as intellectualism, that is to say, the ignoring of any but cognitive elements.† It is against the same view that Mr. Ward's criticisms are directed; and though he prefers to call it presentationism, he is as outspoken in his condemnation of it as Prof. Wundt, whom he claims as an ally.

Now I observe that Mr. Ward uses the word presentationism in two senses, a narrow sense and a broad sense, which he equally condemns. In the narrow sense he means by presentationism the doctrine that all mental states may be resolved into sensations. In the broad sense he means by it the doctrine that psychology has to do solely with conscious events. And what I shall try to show is, that it is only in the narrow sense that Prof. Wundt agrees with him in condemning presentationism; whereas, if the word be taken in the broad sense,

* *Willenshandlung*, p. 62. Cf. *Beiträge zur exp. Psych.*, I. p. 28.

† Philos. Studien, VI. 3 (1890), pp. 387–8.

Prof. Wundt becomes himself a presentationist; in fact, if Mr. Ward realized how truly he is one, we should find him denouncing Prof. Wundt as the greatest and most dangerous of presentationists, instead of claiming him as an ally. This seemingly personal issue will be found, I believe, to involve an important question of psychological principle.

A word, first, in regard to presentationism in the narrow sense, the sense which Prof. Wundt and Mr. Ward agree in condemning. Here I will only say that I am inclined to sympathize with Prof. Münsterberg's critics. He himself admits that every sensation is accompanied by feeling as its inseparable subjective aspect. Every sensation, so the doctrine runs, has a quality and an intensity which are its objective aspect, and which represent the nature and strength of the stimulus, and an emotional tone, which expresses the attitude of the organism towards this stimulus. But it seems to me that the accompanying feeling gets but scanty justice when described as emotional tone. The description overlooks the fact that the feeling has an active as well as a passive side. For though retrospectively and with reference to the sensation it attends we call it pleasure or pain, yet prospectively and with reference to the changes it effects in consciousness it deserves the name of impulse or will. But this is not the question I wish to discuss, which is that of presentationism in the broad sense—or rather the doctrine I find Mr. Ward attacking under that not very appropriate name.

Presentationism in the broad sense is the doctrine, not that "all the elements of psychical life are primarily and ultimately cognitive elements," which is presentationism in the narrow sense, but that all the elements of psychical life are facts of conscious experience, and that "psychology has to do solely with conscious processes and events." This doctrine admits feelings and attention as distinct from sensations and ideas, and also a self which has these feelings and exerts this attention. But it holds that the feelings are facts of conscious experience, that the attention is a fact of conscious experience, that the self is a fact of conscious experience. The very being of feelings, attention, and self, as much as of sensations and ideas, consists in their being facts of conscious experience; if they were not such facts, we should never know anything about them. And since this is so, psychology may restrict itself to the facts of conscious experience, and trouble itself as little with the question of a soul, or of a subject not given in consciousness and without influence upon the course of conscious events, as physics does with the question of material substance. Its proper task is to study the empirical facts and to trace out their con-

nections as physics and chemistry do those of material phenomena. Psychology may, in short, be an empirical or natural science, the science of conscious processes and events.

Now I think it would be safe to say that this general conception of the facts of mind and the duties of psychology with reference to them is shared by practically all of the younger men who take an interest in neurology and in experimental work, many of whom are very far from being presentationists in the narrow sense. I think further that there can be no doubt of Prof. Wundt's entire sympathy with this conception—and no doubt, for that matter, that it is largely from him that the younger men have learned it.* But, for Mr. Ward, a psychology which recognizes only conscious processes and events still falls under the reproach of presentationism. Such a psychology may succeed in explaining nine tenths of the facts, but when it comes to the other tenth, to the subject and its activities, it inevitably breaks down. And so I should like to consider, very briefly, whether Mr. Ward's account of the subject is the only possible account, or whether 'modern' psychology can offer one which shall be adequate to all the facts. I believe not only that it can do so, but that it can offer an account which will not be open to a very serious objection lying against Mr. Ward's, an objection admitted to be such by Mr. Ward himself.

Mr. Ward's account of the subject may be summarized in an introspective observation and three inferences.† First the introspective observation. The facts of mind cannot be properly expressed by saying, 'There are feelings, ideas, volitions,' but only by saying, '*I have* feelings, ideas, volitions,' or more briefly, 'I feel, I know, I will.' Every mental state, in other words, involves a subject by whom it is known or felt or willed. Now the three inferences. First, the subject must be conceived as distinct from the state which it knows or feels or wills. Second, it must be conceived as different in kind from all ideas or feelings or possibilities of such. Third, since all knowledge implies a subject which knows, all feeling a subject which feels, it follows that this subject, just because it is the subject, cannot itself be directly known or felt.

I imagine that the plain man, who began by acquiescing in Mr. Ward's judicious words, will be brought up with a start by this last conclusion. What! he will exclaim, we have no direct knowledge of

* Cf. Philos. Studien, VI. 3 (1890), p. 391.

† Mr. Ward is of course in no way responsible for the form in which I have stated his doctrine. His own best statement of it is that given in his valuable article *Psychology*, in the 9th ed. of the Enc. Brit., p. 39.

XUM

the subject at all? Then how do we ever come to know that there is such a thing? And Mr. Ward acknowledges that the plain man's question is a very difficult one to answer. In fact, he frankly admits, as I have said, that the difficulty of answering it forms a serious objection to his doctrine.

The difficulty is not lightened when Mr. Ward proceeds to draw the further consequence that feelings and volitions, being subjective facts as compared with sensations and ideas, cannot be given in experience any more than the subject. We do not know them directly, we only know *of* them by their effects. If we could know them directly, they would be cognitive states, not feelings and volitions. And here the plain man asks again, If we are not directly conscious of our feelings and volitions, how do we ever learn that there are such things?

Now the 'modern' psychologist surely has common sense on his side when he protests that pleasures and pains and desires and resolves are facts of conscious experience, and that the self too is a fact of conscious experience. But Mr. Ward immediately points out to him that there cannot be an experience without some one who experiences, a feeling without some one who feels; and that consequently, if the self is felt, there must be another hidden self which feels it. Feeling, he insists, implies a subject which is not itself felt.

It seems to me that the 'modern' psychologist's cue at this point is to turn upon Mr. Ward and demand his warrant for the assumption that the subject is not itself felt. This is the very essence of Mr. Ward's doctrine. The doctrine must either be accepted, or the assumption challenged. And I think that the 'modern' psychologist may challenge it with a courageous heart. For what is the source of this assumption? It is an inference from the introspective observation with which we started, the familiar fact of 'I know, I feel, I will.' But is there anything in this fact to justify the inference that the 'I' is not felt? Must we not rather say that the 'I' and the 'know,' the 'I' and the 'feel,' the 'I' and the 'will,' are equally facts of conscious experience? And if an inference is to be drawn, must it not be that all feeling involves a subject which is *also* felt, rather than that all feeling implies a subject which is *not* felt? But if this is the true account of the matter, how comes Mr. Ward to draw his inference that the subject is not felt? I believe that he is led to do so by a preconceived theory of consciousness, a theory not so much extracted from the facts as superinduced upon them.

There are two theories of consciousness. The first conceives it after the analogy of the eye, which sees other objects but cannot see

itself. The other conceives it as analogous to light, which in illuminating other objects illuminates itself also.*

Mr. Ward's is the eye-theory. His principal argument in its favor is that the relation of knowledge logically implies two terms, a knower and a known, and that the knower must needs be distinct from the known, and therefore itself unknown. I should admit that this is true in a sense, but deny that it justifies the eye-theory of consciousness. I should hold that, though the knower is not known, it is nevertheless always experienced or felt, and should rely on concrete examples of knowledge to prove this. In representative knowledge, for instance in memory, that which knows is a present cognitive state, which of course is experienced. And in presentative knowledge, or attention, the self is not prevented from knowing, in the sense of attending, by the fact that it is itself dimly experienced. Mr. Ward's fallacy may therefore be said to lie in applying to conscious experience or feeling an analysis which holds good only for knowledge. As for the other argument, if it is another, that mental states are phenomena, or appearances, and must therefore appear to something, I should reply that it begs the question. For whether mental states are in the proper sense of the term phenomena is precisely the question.

The great objection to the eye-theory is, however, the difficulty already mentioned in regard to the knowledge of the subject. If the subject is not directly experienced or felt, it is impossible to understand how we ever learn of its existence. To my mind this is not merely an objection : it is a refutation.

The difficulty referred to is sometimes evaded in the following way. Though the subject cannot know itself at the moment when it knows, it is assumed that it can turn and know itself the moment after. Prof. James's line of argument on this point is so instructive that I cannot forbear reproducing it here.† After getting happily rid of the soul, and identifying the knower as the passing state, he finds that this state, just because it knows, cannot also be an object of knowledge. Thus the present moment of consciousness, instead of being the lightest in the series of mental states, becomes the darkest. Indeed, it is not an empirical fact at all. Only after it is gone have we any knowledge of it. As it vanishes it becomes the object of a new unknown state. It thus appears that we have no direct evidence of the existence of mental states while they last; they are not verifiable facts. For once, and once only, one is tempted to regret that Mr. James is such a devout reader of Mr. Shadworth Hodgson.

* Cf. Wundt, *Logik*, II. 502 ff. † *Principles of Psychology*, I. p. 304.

Such difficulties and absurdities as these are the inevitable outcome of the eye-theory. In Mr. Ward's account, the self, the real efficient one, loses its status in consciousness and becomes a quasi-transcendent entity, little better than the soul in disguise. Our knowledge of it is a sort of standing miracle. We are cut off from ourselves: an impenetrable curtain is drawn between us and that which should be nearest and most familiar. In Mr. James's, we are told that the lamp of consciousness is dark while it burns, luminous only after it has gone out; that we cannot see it while it is present, but may do so as it begins to be absent; in short, that we may remember and reflect upon that which we have never experienced. The only escape from these absurdities and difficulties lies in breaking with the eye-theory, and putting the light-theory in its place. But can the light-theory explain the self and its activity in attention? This is the question that now calls for an answer.

An attempt to solve the problem of the self and its activity in accordance with the light-theory, the only such attempt in fact with which I am acquainted, is Prof. Wundt's much-maligned theory of apperception. This theory has been strangely misunderstood. It has oftenest been regarded as a form of the eye-theory, or even as a crude relapse into the old faculty psychology. Yet there is not the slightest ground for either of these assumptions, and I think it may be shown that the theory is perfectly consistent with the conception of psychology as a science dealing solely with conscious processes and events. In fact, one of the greatest merits of Prof. Wundt's psychologizing seems to me to be the consistency with which he holds to this conception. It is usual for psychologists to say that they have not been able to understand the theory of apperception. I believe that this is true both of Prof. Münsterberg who combats it, and of Mr. Ward who appeals to it against presentationism. Prof. Münsterberg combats it because he supposes it to contain non-empirical elements; whereas his own impersonal subject-Ego is a non-empirical element of the most obvious kind, a relic of the eye-theory, which might to the advantage of his psychology be altogether dropped.* Mr. Ward, on the other hand, supposes that the theory of apperception is a form of the eye-theory, like his own doctrine of the subject, while in truth it is the legitimate outcome of the light-theory.

The theory of apperception, then, is an attempt to solve the problem of the self and its activity in terms of the light-theory. According to Prof. Wundt, we have not two selves, an unknown subject and an

* Cf. *Beiträge zur exp. Psych.*, I. p. 38, and esp. p. 55: "So wenig wie die Netzhaut sich selber sehen kann. . . ."

objective self-consciousness,* or an impersonal subject-Ego and a personal object-Ego,† or a 'self as knower' and a 'self as known'.‡ We have but one, the self which is a fact of every one's conscious experience, the empirical self. It is the empirical self which attends —that is, determines what shall be attended to. But what shall be attended to depends on the contents of our minds, on the totality of our latent ideas—on these, however, not merely as ideas, but as accompanied by feeling—in other words, on our likes and dislikes, on our interests. These constitute the very essence of ourselves. The self may therefore be defined as the manifestation in consciousness of our latent ideas, of our likes and dislikes, of our interests, in so far as these are operative in determining what shall be attended to.§ According to Prof. Wundt, they are manifested in a state of fusion, under the form of a 'total feeling,' which occupies the background of consciousness.‖

Not only does the self, as thus conceived, determine what shall be attended to, but it does so actively; and we may rightly speak of its activity, and call it an agent. For the self is a relatively permanent fact; it has much the same permanence that belongs to a material object. And if we are justified in speaking of the sun as active when it melts wax, or of the wind when it moves the sails of a windmill, or of the windmill when it grinds corn, then to the same extent we are justified in speaking of the self as active when it attends.¶ Indeed, it is only so far as the latent ideas are active in attention that they are called the self.**

It will be evident that there is a wide difference between the relation of the self to the idea attended to and that of the eye to the object it sees; for the eye can catch no glimpse of itself, whereas the self which attends, though not in the focus, is yet in the fringe. Thus Prof. Wundt, after insisting that the facts of mind are one and all conscious events, proceeds: "Above all the percipient subject is not an independent spectator standing over against its own ideas, as under the treacherous figure of external sense-perception it is here represented, but forms an inseparable constituent of the psychic process itself."†† The reader may judge from this passage how far Mr. Ward is justified in claiming Prof. Wundt as an ally.

If the above account of the self is correct, there is evidently no

* Cf. Dr. Ward's article. † Cf. Münsterberg, *Beiträge zur exp. Psych.*, I. p. 55.
‡ Cf. James, *Psychology*, Briefer Course, pp. 176 and 195.
§ *Vorlesungen über Menschen u. Thierseele*, 2d ed. (1892), pp. 248–9, 269.
‖ Philos. Studien, VI. 3 (1890), pp. 392–393.
¶ *Vorlesungen*, p. 245. ** Ibid., p. 269.
†† Philos. Studien, VI. 3 (1890), p. 389.

ground for denying that feelings and volitions are facts of conscious experience because they are subjective. Nor is there any ground for denying this because they are cognitive states. For a state is cognitive, not because it is known, but because it knows; a memory, for instance, is cognitive because it knows a past experience. But cognitive states, like all others, are in their first intention facts of conscious experience; therein lies their immediate being. Mr. Ward's denial, in short, that we consciously experience our feelings and volitions is in reality, what he says it will seem, an extravagant paradox.*

If it be asked whether we can attend to our feelings, I should answer that we attend to them in attending to the cognitive states of which they are the subjective side. And the same remark applies to attention. Although what we ordinarily mean, when we speak of attending to attention, is that we remember states just past and attend discriminatively to the aspect of attention in them. This would, in fact, be my solution of Mr. Ward's puzzle about 'consciousness of consciousness' or reflection.

If, finally, it be asked whether we remember our feelings, I should say that we undoubtedly do as a matter of fact, but always in connection with the cognitive states of which they are the subjective side. I do not mean by this that past feelings are remembered by means of present feelings. Such an assumption would indeed make feelings cognitive. What I mean is that every experience may leave behind a representative idea of itself, an idea which will be cognitive of the emotional aspect as well as of the cognitive aspect of the experience, but which will itself be attended by a new feeling.

Sensations and ideas, then, feelings, acts of will, and the self as well, are all facts of conscious experience, and in this they have their being. The self with its feelings and activities is consubstantial with all other mental facts, and is known in the same identical way.

A chief point in the Kantian philosophy is the proof that we have no direct and intuitive knowledge of other beings than ourselves, but can know them only representatively, through the medium of our own conscious states. This doctrine seems to interpose an opaque barrier between us and the things we would know, and to make anything like a full and satisfactory knowledge of them forever impossible. Is our idea of an intuitive knowledge of things, then, nowhere realized? Are we as ignorart of ourselves as we are of other beings? Do "all the great realities escape us"? It seems to me that the Kantian doctrine requires a complement. Representative knowledge holds for

* Enc. Brit., 9th ed., art. *Psychology*, p. 44.

other beings, but it does not hold for ourselves. In conscious experience we have the realization of the demand which in the case of other beings was judged unattainable, the demand to know things as they are in themselves. Conscious experience is not less than knowledge, but more. In it we immediately grasp ourselves and know ourselves for what we truly are. Conscious experience is, in brief, the one point at which we come into immediate contact with reality.* It seems to me therefore that the philosopher is not less concerned than the psychologist to hold fast to the principle that there is nothing in consciousness of which we are not conscious, nothing in experience which is not experienced.

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* Cf. Paulsen, *Einleitung in die Philosophie* (1892), p. 377.

PSYCHOLOGICAL LITERATURE.

EDUCATIONAL.

The Science of Education: *Its General Principles deduced from its Aim.* By JOHANN FRIEDRICH HERBART. Translated from the German, with a Biographical Introduction, by HENRY M. and EMMIE FELKIN. pp. 268. Boston: D. C. Heath & Co. 1893. Price $1.00.

Apperception: A Monograph on Psychology and Pedagogy. By Dr. KARL LANGE, Director of the Higher-Burgher School, Plauen, Germany. Translated by members of the HERBART CLUB, and Edited by CHARLES DE GARMO. pp. 279. Boston: D. C. Heath & Co. 1893. Price $1.00.

The interest in Herbart's psychology is to-day based almost wholly upon its applications in education. This interest is not only widespread, but growing. In America it has reached very respectable proportions. In England it is gathering force. Even in France it is discernible. The reason for this is not far to seek. Ever since the conviction was established that sound educational theory must find its justification and support in psychology, there has been a searching for light. Bain and Herbert Spencer contributed something, and Sully a great deal; yet Herbart contributes still more. It is probably true, as De Garmo has said, that no other system of psychology assigns to the teacher so important a function as Herbart's, and no other is so well adapted for practical application in the work of education. Proof of this is to be found by measuring the success of its use in the elementary schools of Germany, and, under the stimulus of Frick, in some secondary schools as well.

The two volumes under notice are translations of material that has been found suggestive and useful in Germany by followers of Herbart. The first shows us Herbart himself, and the second analyzes what has come to be regarded as his main doctrine, that of apperception. In both works the purely psychological element is subordinated to the educational applications. They belong therefore to the field of applied or, as it is sometimes called, educational psychology. It may be said at

the outset that both translations seem to be unusually well done, and that the thought has lost nothing—indeed at some points it has distinctly gained—by transference to the English.

Herbart cherished the individual and would not crush him under the weight of the mass. The individual's development, particularly on his active side, is constantly before Herbart as the educational aim. Government (*Regierung*), instruction (*Unterricht*), and discipline (*Zucht*) are the three educational stages or moments. They co-operate to produce intelligent character. In the discussion of each he combines the practical with the theoretical and formulates rules of procedure in dealing with the growing mind. If some of the analyses yield results that are almost too precise and if portions of the praxis are so formal and well-fitted as to appear mechanical, the blame must be laid on the psychological presuppositions on which they rest. For many of these are mechanical to the last degree. Yet the suggestiveness of the treatment given by Herbart is not to be denied or minimized. It has done and is doing an important work in the schoolroom.

Lange's exposition of apperception is much more psychological, though of course his eye is always fixed on educational practice. The assimilation of a perception is apperception, and in the process of being assimilated the perception gains in clearness. In a brief historical sketch, Lange shows that the idea of apperception is not an artificial one invented for the purpose of giving a new and more learned appearance to well-known educational maxims; but that, on the contrary, it has been under discussion since Leibnitz. Its meaning for Herbart is contrasted with the use of the term by Kant, and with the later theories of Lazarus, Steinthal, and Wundt. Admirable accuracy and precision characterize this portion of the book.

On the purely educational phases of these books this is not the place to enlarge. My purpose has been simply to call attention to them as indicating a tendency in the educational applications of psychology.

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THE NERVOUS SYSTEM.

1. *Zahl und Vertheilung der markhaltigen Fasern im Froschrückenmark.* J. GAULE. Leipzig, 1889.
2. *Développement des Éléments de Système Nerveux Cérébro-Spinal.* W. VIGNAL. Paris, 1889.
3. *The Structure and Combination of the Histological Elements of the*

Central Nervous System. F. NANSEN. Bergens Museums. Aarsberetning, 1891.

4. *Ueber einige neuere Forschungen im Gebiete der Anatomie des Centralnervensystems.* W. WALDEYER. Leipzig, 1891.
5. *Die Funktionen der Ganglienzellen des Halsmarkes.* O. KAISER. Leiden, 1891.
6. *A Microscopical Study of Changes due to Activity in Nerve-cells.* C. F. HODGE. Journal of Morphology, VII. 1892.
7. *Nuevo concepto de la Histologia de los Centros Nerviosos.* RAMÓN Y CAJAL. Revista de Ciencias Medicas de Barcelona, XVIII. Nums. 16, 20, 22, and 23. 1892.
8. *The Structures and Functions of the Brain and Spinal Cord.* V. HORSLEY. Fullerian Lectures for 1891. London, 1892.
9. *A Physiological, Histological, and Clinical Study of the Degeneration and Regeneration in Peripheral Nerve-fibres, after Severance of their Connections with the Nerve-centres.* W. H. HOWELL. Journal of Physiology, XIII. No. 5. 1892.
10. *Notes on the Arrangement of some Motor Fibres in the Lumbo-sacral Plexus.* Plates 20, 21, 22, 23. C. S. SHERRINGTON. Journal of Physiology, XIII. No. 6. 1892.
11. *Der feinere Bau des Nervensystems im Lichte neuester Forschungen.* M. LENHOSSEK. Berlin, 1893.
12. *The Nerve-cell considered as the Basis of Neurology.* E. A. SCHÄFER. Brain. 1893.
13. *The Arrangement of the Sympathetic Nervous System, based chiefly upon Observations upon Pilo-motor Nerves.* J. N. LANGLEY. Journal of Physiology, XV. No. 3. 1893.

In a recent paper Prof. Schäfer (12) has made some very useful suggestions concerning the nomenclature. We look upon the so-called nerve-fibres as outgrowths of the nerve-cell. Prof. Schäfer suggests that, since in all other cases we make the term *cell* cover not only the principal mass of cytoplasm with its nucleus, but also all the processes of this mass, it would be advantageous to be consistent in this case and, in describing the elements of the nervous system, to use the term cell in an inclusive sense.

In 1891 Prof. Waldeyer (4) in his admirable review of the recent studies in the anatomy of the central nervous system proposed the term *neuron* for the nerve-cell and its processes. This term was so convenient that many have already accepted it; but there is more to be said in favor of Schäfer's suggestion, which we here adopt. The processes of the cell are by him designated as *neurons*—so far as they

form the axis-cylinder of the nerve-fibres,—or *dendrons*—so far as they form the protoplasmic prolongations. The examination of nerve-cells has been most successfully carried on by the methods of Nissl, Ehrlich, Weigert, and Golgi. Improvements in the technique have increased the number of processes which can be demonstrated in connection with the cell-body. The exact meaning of these processes is still open for discussion. The method of Golgi shows that in a typical cell, such as one from the human cerebral cortex, there are differences of form and outline which clearly mark the dendrons from the neuron. But as we descend the vertebrate scale, the two sorts of processes come to look much more alike under this treatment. This at once suggests that in the lower vertebrates the dendrons may be more similar to the neuron physiologically also (7). The older histology taught that many nerve-cells had but a single neuron which was unbranched and the function of which was to carry impulses from the cell-body to their destination. It also recognized bipolar nerve-cells which had two neurons, such as are found in the spinal ganglia of the fish, but the effort was not made to reduce these apparently divergent forms to a common type.

In the spinal ganglia of mammals we find a cell which during its development is at first dineuric (i.e., has two neurons), and later by the fusion of the two for a short distance into a common stem becomes mononeuric. This stem so formed, may be looked on as having two branches, along one of which nervous impulses arrive and by the other of which they leave the cell, each branch being continued as a portion of the single neuron which unites both with the cell-body. There is no reason that these two branches should be of the same size, and thus it is possible that all apparently mononeuric nerve-cells receive the incoming impulses by way of one or more of these branches, so called (11). Thus it is possible to reduce all nerve-cells to one type. It seems necessary that we should have some such pathway into the cell-body as well as one out of it. In some cases the surface of the nerve-cell is covered by minute disks which appear to be the enlarged termination of fine fibrils surrounding it. This arrangement suggests that impulses arriving by that fibre may in this way pass to the cell-body.

For the most part the neurons from elsewhere break up into fine brushes in the neighborhood of the cells among which they end; and just how the impulse passes from one nerve-cell to another is thus a matter of much speculation. The mass of evidence—not all—is to the effect that under ordinary conditions we do not have continuity between one nerve-cell and another, and, that being the case, the difficulty is to show how the nervous impulse crosses the gap. But the

other alternative is always open to us, namely, to question the reality of the gap. The nerve-cell of the day, then, has a cell-body and processes which may be more or less different. When well differentiated these processes are assumed to be of two sorts. The dendrons are contrasted with the neuric processes, and of the latter there may be one or more. The length of the neuric process may range from a small fraction of a millimeter to more than a meter. It ends, however, always in a brush (7). The cell-body, the neuron, and the brush-like termination form the three important subdivisions.

One chief difference between cells lies in the length of the neuron. When this is very short, the brush is formed close to the cell-body and within the central system, and such cells appear to be interpolated in the pathways of the surrounding neurons. Wide study of the nervous system in different forms shows that the general arrangement of the elements is that of a series of cells so placed that from one cell-body a neuron passes to the neighborhood of another cell-body and there terminates in its brush, and so the units repeat themselves to the end.

When a group of cell-bodies is located at a point where incoming neurons terminate, each neuron may have connection with a number of cells, and in this way the impulse arriving at any point may pass on by either a number of paths or by one of the several paths open to it. Here we reach a fundamental fact, the explanation for which is obscure. It is plain that the possible pathways for an impulse within the nervous system are very numerous indeed. For example, any voluntary muscle can be contracted in response to any possible stimulus from any part of the sensorium. The arrangement which permits of this must be highly complicated. In view of this complication, it has even been suggested that the cell-body is, so to speak, a bystander, and that the physiological reactions go on in the branches of the neurons, the cell-body itself acting simply as a nutritive centre (3); but the idea has little to support it. Indeed, the size of the nerve-element has, without doubt, some connection with the energy it can put forth. Where a group of nerve-cells controls a limb, it is found to be the larger cells that are associated with the more proximal and more bulky muscles, which are at the same time those that do the most work. As the result of exercise, it is the nerve-centres that first run down, and muscular contractions cease because in some way the nervous impulse has changed. The size of cells increases so long as general growth continues; and in those animals in which growth is practically indefinite the range may be very great. In man, we consider that the formation of nerve-cells ceases some time before birth, so that the individual comes into

existence with marked limitations regarding the possible number of elements composing the nervous system (1). The existence of this numerical limitation seems to offer difficulties in explaining the marvellous capacity for development, through exercise and training, which is so peculiar a character of the central nervous system. These difficulties are lessened, however, when we remember that by no means all these possible nerve-elements undergo further development.

Vignal's (2) study of the development of the nerve-cell shows in a given species of animals more and more cells that are fully formed and characterized with advancing age; and Kaiser (5), from the study of the spinal cord, shows that in man between birth and maturity the number of well-developed nerve-cells is doubled. There is no evidence that these cells are newly formed, but there is some evidence that they are from the first present *in petto* and developed in serial order.

Concerning the undeveloped nerve-cells between birth and maturity, we have much to learn; and, curiously enough, the current histological methods bring out these structures but poorly. There is another standpoint from which size is of interest. It has long been recognized that in man the male was possessed of the heavier central nervous system. This has been interpreted as indicating the mental superiority of the male. It is not our object to enter into a controversy in which there are so many vested interests, but to point out simply that among males the taller individuals also have a decidedly greater brain-weight than the shorter ones, and that another curious weight-relation exists, of such a nature that, when we compare the brains of the two sexes,—first from persons on the limits of intelligence, and second from those with just enough brain to keep alive, though idiotic,—we find in both cases the average weights less for the female than for the male. In these instances we may fairly assume that lack of complexity in structure is at the basis of the intellectual condition; and if such were the case, we could most easily explain the differences in the gross weight of the brains as due to differences in the size of the constituent elements. This, however, leaves the size of the elements still to be explained. Possibly when the experiments of Hodge (6) shall have been more extensively applied, we may get assistance from the comparison of the way in which small and large cells change as the result of exercise. Hodge finds that fatigued cells have changed their shape and grown smaller. Whether this change in the shape of the cell and its nucleus is simply the physical result of the shrinkage, or whether it is a remnant of the capacity for contraction, is a point that would bear examination.

Schäfer (12) suggests that the rhythmic manner in which central nerve-cells can be shown to discharge may be the result of a series of contractions which, if the axis-cylinder of the neurons be considered as made up of a series of tubules, might be supposed to give rise to variations in the surface-tension of the fluid substance within these tubules. The passage of this wave of varying surface-tension would then coincide with the passage of the nervous impulse. If this were the case, it should be possible to obtain both reinforcement and interference of two impulses. The tubular character of the cell-contents has been contended for by Nansen (3), but it is hardly probable that the structure of the nerve-cell is unique; and if this tubular character is genuine, indications of it should be found elsewhere. Again, the physiology of the nervous system is complicated by the fact of multiple innervation, in the sense that afferent and efferent fibres from more than one centre are distributed to the same region. The return of motor functions after injuries to the cerebral hemispheres or to the spinal nerves, the reactions of the pilo-motor nerves, all point to the fact that there is a preferred path for the impulses under normal conditions; but this may be by no means the only path (8, 9, 10, 13).

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APHASIA.

Ein Fall von Seelenblindheit und Hemianopsie mit Sectionsbefund. HERMANN WILBRAND. Deut. Zeitschr. f. Nervenheilkunde, II. 5 u. 6. 1892.

Ein Fall von aphasischen Symptomen, Hemianopsie, amnestischer Farbenblindheit und Seelenlähmung. E. BLEULER. Archiv für Psychiatrie, XXV. 32. 1893.

Die Bedeutung der Aphasie für die Musikvorstellung. R. WALLASCHEK. Zeitsch. für Psychol., VI. 8. Sept. 1893.

The study of the different forms of disturbances of speech, and of the various processes of disease in the brain which are capable of producing them, has thrown much light upon the normal action of the brain in thought. And as new forms of aphasia are discovered and the defects which they imply in mental processes are more closely analyzed it is quite clear that considerable insight can be gained into the mechanism of thinking. Before proceeding to review some recent observations upon aphasia it may be well to present a brief summary of the facts so far as they are already determined which are accepted regarding the physical basis of speech.

It is now known that as the physical basis of any word, be it noun

or verb, there is a series of mental images acquired through different senses, located in different regions of the gray cortex of the brain, and joined together in a unit by a series of association-tracts which pass in the white matter under the cortex. The word "concept" long used by psychologists to denote congeries of mental images making up an idea conveyed by a single word may be adopted by the pathologist to indicate this collection of mental images. To be complete, such a concept must have all its parts intact and the connections between those parts also intact.

If we take such a word as 'book' or 'rose' as an example and watch the process going on in the mind of a child as it acquires a primary knowledge of some particular book or rose, we at once see that this particular concept has a limited extent, consisting of the mental images of the object (1) as seen (form, color), (2) as felt (shape, size, temperature, hardness), (3) as smelt, (4) as tasted, if the object has odor or taste, (5) as heard, if the object is audible. These five mental pictures comprise the concept of the object, and the separate mental images are associated with all the others, so that when one arises in memory it inevitably recalls all the rest.

If we take as another example the verbs 'to run,' 'to sew,' we call up to the mind memory-pictures of some individual in action or of some act of our own with its attendant sensations; and thus as the basis of any verb, as well as of any noun, we must think of a congeries of mental images closely associated with one another, forming a mental picture.

So far our analysis of the basis of concepts would apply equally well to a child who had not learned to speak, to a deaf-and-dumb person, or to a healthy man. In the man, however, who can talk, there has been added to this original concept a "word-concept" quite similar in its parts to the other and consisting of an image of the word (1) as heard, (2) as seen in print or writing, (3) as pronounced by muscular effort, or (4) as produced in writing by movements of the hand. Each of these separate word-images is joined with the others, and each part of this word-concept is also connected with every part of the mental concept which the word enables us to convey to others. Each one of these various mental images is known to have a separate location in the brain. Thus it becomes evident that the psychological term "concept" (German *Begriff*) stands, not for a simple thing, but for a very complex thing, having as its physical basis an activity not only of widely distant gray cortical areas of the brain, in each of which a separate memory-picture is located, but also of long or short association-tracts, running in every conceivable direction between these vari-

ous areas. If there is such complexity in the physical basis of so simple a concept as a particular book or rose or as a simple act like running or sewing, it is evident that the physical basis of more complex concepts such as the class books or roses or the complex acts implied by the verbs 'to educate,' 'to civilize,' is very difficult to fathom. It becomes equally evident that for the conduct of thought or the wholesome action of the mind in dealing with concepts, be they simple or complex, an integrity of the entire brain is necessary both in its gray cortical extent and in the white subjacent association-tracts.

Small lesions of the gray cortex in various parts may injure these concepts by depriving them of some constituent mental image, and small lesions in the white matter may disturb the use of these concepts by dissociating images which should be closely bound. Hence it is evident that lesions in various parts of the cerebral hemispheres will produce disturbance in the use of our mental concepts, and this may be manifest either as an inability to recognize by any sense places or objects, the conditions known as mind-blindness, mind-deafness, and mind-paralysis; or as a condition of aphasia, of the form of word-blindness, or word-deafness, or inability to speak or to write.

The localization of the various lesions producing such conditions is a subject apart from our present purpose; it is only to be said that from the condition present a very precise conclusion as to the part of the brain affected can be reached.

But while the various well-marked forms of aphasia indicate large lesions in various situations, many particular cases of aphasia cannot be assigned to any one of these forms, but present very interesting features. When, for example, a person loses the power of reading, yet can copy any word which he sees, and in the act of copying becomes conscious of the meaning of the word which he is writing, being thus able to read by means of his muscular sense, it is evident that he has not lost the visual memory-pictures, but that it has been possible to awaken them in consciousness only in an unusual manner. This may be likened to a break in a railroad which compels a passenger to reach his destination by a roundabout route with one or more changes of cars, instead of by the direct road. It is the study of these partial forms of aphasia which is now exciting interest. It may be stated that they are usually produced by small areas of disease in the subcortical white matter of the brain—in the association-tracts, which cut off the connection between various mental images forming the concept.

Wilbrand has described a most interesting case of mind-blindness with word-blindness in which a small region of disease affecting chiefly the association-tracts in one occipital lobe of the brain was found.

His analysis of the defects of speech and thought produced by the break in these association-tracts is too elaborate to be reproduced here. He shows, however, that the area of the brain concerned in the reception of visual impressions and that in which the visual memories are stored are distinct from one another, but are closely joined by associating fibres. If these fibres are broken, the sight of an object no longer brings up its memory and there is no conscious recognition of a thing when it is seen. But the recollection of an object may occur not only by means of seeing it but also by a train of ideas or by hearing its name. Hence the break in the one association-tract does not necessarily prevent the individual from recollecting the object or calling its memory-picture up to consciousness when it is spoken of or when the train of thought leads to it. On the other hand, a break in the association-tract joining the visual memory-picture of the object with the memory-picture of its name results in an inability to call to mind the object when its name is heard or to name an object which is recognized when seen.

Bleuler, in a very carefully written article upon a case of amnesic aphasia with word-blindness and mind-blindness, discusses many of the processes of association and their defects. He calls attention to the fact that it is rare for an entire concept to be obliterated from the mind, because its parts, being widely scattered through various regions of the cortex, are not all involved in a single lesion. And it is easy to call up to the mind any concept in various ways, as numerous associations are capable of arousing some one of its parts and thus leading to the whole. On the other hand, it is common to find a single part affected by disease, e.g., names of objects. But this amnesic aphasia has its degrees, from the physiological inability to recall a name, up to complete loss of memory. Even in the latter there is in many cases rather a hindrance in finding the words than a loss of words; the acoustic word-picture is not lost, for when the word is heard it is recognized. Bleuler asks, Why is the way open from the ear through the word-memory to the concept, when it is closed from the concept to the word-memory? Why will a man call to mind a person whose name he hears, yet be unable to name a person about whom he has a knowledge perhaps quite complete? The difference appears to lie in the course taken by the associating impulse: in the one case from a small region of the cortex outward to a general centre or rather collection of centres, in the other case from this collection of centres to a particular area of the cortex. The first he likens to the way taken by a fish down a brook and river into the sea, easy to follow down the current. The second he likens to the difficulty the fish

would find in coming from the sea to the particular little brook from which it started. Or, to take another illustration, it is easy to get from a particular town into a neighboring country, but much more difficult to reach that town from any part of the country. If the associations between various parts of the concept are not entirely intact—if the concept is injured—the normal easy flow of associating processes to one focus in the word-hearing memory is interfered with, and there results a difficulty in calling the word to mind. Thus amnesic aphasia may occur when lesions are purely subcortical and the word-memory is actually intact. In daily life the word easily evokes the idea, but ideas are more difficult to state in words. In the first case we go from the particular to the general, in the latter from the general to the particular. The keenest subjective mental analysis therefore is inferior to the study of such cases of disease in throwing light upon the actual physical mechanism of thought.

Wallaschek has shown that disturbances in musical expression and appreciation, quite similar to those in speech and its understanding, have been observed. These may or may not be attended by aphasia. He distinguishes several forms of this condition, which he names amusia, viz.: I. Disturbance in singing: (*a*) motor amusia, in which the person can no longer produce a note or tune; (*b*) sensory amusia or tone-deafness, in which the person cannot recognize various tones or tunes; (*c*) paramusia, in which the effort to sing is unsuccessful because wrong notes are struck; (*d*) musical amnesia, in which a person can sing with or after another but cannot sing alone correctly. Thus a well-known Wagner-opera singer who was suffering from incipient softening of the brain demanded of the manager that some one in the wings should whistle the notes he was to sing, as without help of this kind he could no longer remember the notes or their succession. This occurred prior to any disturbance of speech. Other singers have suddenly lost for a time all memory of music. II. Disturbance in writing music may occur independently of loss of the power of writing words (musical agraphia). One person has been seen who could write music when he had lost the power of writing letters. III. A loss of ability to read music has been observed in one patient (musical alexia). IV. Inability to perform on a musical instrument formerly used has been noticed when other movements were preserved (musical amimia).

Numerous interesting examples of these defects are given in the article. The reviewer can confirm some of its statements. A patient now under his observation with total loss of power of speaking, the understanding of speech being preserved, is being successfully taught to sing in a high pitch words which he cannot be taught to say. In

many cases of aphasia the condition of amusia is also present, but their separate occurrence proves conclusively that the two functions are performed by distinct parts of the brain. This is confirmed by the fact that infants may learn to sing a melody before they learn to talk. Wallaschek further calls attention to the numerous association processes which music awakens. Few can hear a melody without keeping time by some movement or by silently humming the tune to themselves. For some the association of music with motion is very close and musical memories may often be of a motor kind. Persons may commit to memory the succession of finger positions on a piano which are necessary to play a piece and be then able to render it when their knowledge of music is too slight to allow them to play by ear. Some can only reproduce a melody of which they know the words, the latter awakening the former; others, while able to recall the words in the act of singing, cannot do so without the tune. The author attempts to group persons into types—'visuels,' 'auditifs,' and 'moteurs'—after Ribot, in respect to their predominant kind of musical memory. And he intimates that the various theories as to the origin of music, some regarding it as a method of speech, others as a method of dramatic action, others as a method of emotional expression, may be traced to the individual variations of the theorizers. It is possible that the old controversy regarding the 'universal' in logic—which divided the schoolmen into nominalists, conceptualists, and realists—may be similarly solved. The article closes by an attempt to show that a musical person differs from a non-musical person rather in the superior number and activity of his association-processes than in any distinct quality of mind.

It is evident that in the later studies of aphasia the importance of a study of the processes of association is being appreciated.

M. A. S.

HYSTERIA, PARAMNESIA.

On the Psychical Nature of Hysterical Unilateral Amblyopia and Sensitivo-sensorial Hemianæsthesia. Professor BERNHEIM. Brain, Parts LXI. and LXII. 181–190. 1893.

Taking the case of a youth of 19, the upper left half of whose body was completely anæsthetic, Bernheim defends the view that the insensibility in such hysterical cases is not real, but only mental, just like that produced by suggestion in hypnotic subjects. The boy's left eye, used alone, appeared both dim-sighted and color-blind, but varied so from day to day that one was led to believe 'that the

imagination played a part in the results." Experiments with a prism and with Snellen's apparatus (red and green letters used as objects and eyes armed with red and green glasses) proved that under these conditions, both eyes being used together, each of them saw its own proper letters distinctly. Moreover, under certain unusual experimental conditions employed for testing the acuteness of sight, the left eye used alone was able to read large print easily at a distance of five metres, but not within that distance. The mind, disturbed by the new conditions, says Prof. Bernheim, omitted to inhibit the sensations received, and the auto-suggestion arose, to see at the distance of five paces. Once surprised into seeing, the eye for a while kept up the habit under these conditions. The patient moreover called hot cold, black white, and *vice versa*—results only to be explained by perverted imagination. He had no resident sensations in his left arm, and with eyes closed could not find his left hand with his right one. The suggestion that the one was a magnet attracting the other enabled them to find each other; but on this being explained aloud to a clinical audience, the effect ceased, and the youth relapsed into the old symptoms. His left ear was stone-deaf, but could be surprised by artifice into hearing. When he discovered the artifice he over-corrected himself and then appeared deaf in his right ear also. All these results, which suggest shamming, and would make most examining physicians unhesitatingly condemn such a patient for an impostor, are explained by Bernheim as effects of 'auto-suggestion,' analogous in all points to the systematized anæsthesias which suggestion produces in hypnotic subjects, and in which it is now well proved that the patient must by one part of his consciousness pick out and recognize the object which it is the duty of the other part of his consciousness to ignore. Bernheim insists that all hysteric anæsthesias are of this type, and that the contraction of the field of view, for example, which they so generally present is a symptom which it is impossible that ignorant patients could deliberately conspire with such unanimity to simulate. Simulation is present, if you like, but it is unwitting and involuntary, and the patient's own consciousness is its first victim.—One is reminded of Mr. Myers's phrase that hysteria is a 'disease of the hypnotic stratum.'

W. J.

Des Paramnésies. A. LALANDE. Revue Philosophique, XXXVI. 485. Nov. 1893.

This name has been given to the very common illusion of feeling as if one had already undergone the experience which may be passing,

already been with just these people, in just this place, saying just these things, etc. Usually a strong emotion accompanies the sense of recognition. M. Lalande says that of 100 persons interrogated, 30 know the phenomenon, and that the completeness of detail in the recognition makes it impossible to confound it with a mere judgment of imperfectly discerned resemblance between the present and a real past situation. He also notes, and gives several cases of, the asserted fact that the recognition of the situation sometimes goes so far as to lead to a correct expectation of what the next-following details are to be. The phenomenon is too wide-spread to be considered pathological. Both sexes and all ages and temperaments present it. Passing to its explanation Lalande rejects Wigan's theory of the non-synchronous action of the hemispheres, and Anjel's of a tardy act of *perception*, which, when accomplished, looks back on the just previous *sensation* as if it were another perception remotely past. He himself gives a first explanation similar to Anjel's, by assuming momentary absences of mind with an exaggerated impression of the duration of the lapse, and a return to the object, which thus appears to be seen after a considerable interval and for the second time. But admitting the insufficiency of this theory, he proposes another more original one based on subliminal or 'unconscious' telepathic perception. This perception, if *made conscious* by the succeeding mental state, might give rise to the sense of a previous experience repeated: "Thus, I am walking with a friend, and he thinks of something which he is going to say. A telepathic sensation occurs in me, and I perceive directly the interior thought by which he has thought his phrase. But this perception, to which I am unaccustomed, remains unconscious unless the phrase is actually pronounced. If, on the contrary, he pronounces his phrase, the auditory sensation may awaken in the obscure recesses of my mind the identical perception which I have just received; I shall seem then to recognize it, or rather I shall really recognize it. The only illusion is that of projecting my remembrance into a more or less remote past, in order to account for the confused character which comes only from its origin." This theory, says its author, would also account for the element of prevision. "In any case," he adds, "the key to paramnesia must be sought in the existence of a double perception, unconscious at first, then conscious." W. J.

THE PERCEPTION OF LIGHT AND COLOR.

On a Photometric Method which is independent of Color. OGDEN N. ROOD. Am. Jour. of Science, 3d Ser. XLVI. 173–176. 1893.

Prof. Rood has rendered a very important service to those who are interested in theories of color-vision by devising a simple and certain method for determining the brightness of colored papers. The only methods that have yet been devised consist in simply putting the question to consciousness, does this or that surface seem to be the brighter? Prof. Rood's plan consists in mixing the papers to be compared upon the color-wheel, and observing whether the sensation of flickering is produced or not; that sensation diminishes in intensity the more nearly the papers are alike in brightness, and ceases only when (as tested by the ordinary means) the difference in brightness is reduced to $\frac{1}{60}$ of the total light. To test the method, the brightness-values of six disks (three pairs of complementary colors) were obtained by it; the complementary colors were then mixed in the proportion necessary to produce gray, the brightness of the resulting gray was calculated, and compared with its brightness as observed by the ordinary method. The differences were found to be very slight.

There is this interesting psychological difference between this method and the direct one. When one endeavors to decide by simple inspection which is the brighter of two colors widely different in tone but not in brightness, a disagreeable feeling of uncertainty and embarrassment is experienced (*cf. Physiol. Optik*, p. 428). Helmholtz says: "I must personally repeatedly declare that I can hardly trust myself to form a judgment upon the equality of heterochrome brightnesses. . . . I have always the sense-impression that it is not a question of the comparison of one quantity, but of the combined effect of two,—brightness and color-glow (Farbengluth),—for which I do not know how to form any simple sum, and which I also cannot scientifically define" (p. 440).* Upon Prof. Rood's method, on the other hand, the moment of deciding that the two brightnesses are equal would seem to be a moment of certainty and agreeableness,—"no shock is experienced and the colors are seen to mingle in a soft streaky way."—As regards the accuracy of the determination by the old method,

* It will be noticed that this is a most important statement, on the part of Helmholtz, for those who believe that what we speak of as the brightness of a color is, in fact, a sort of combined voluminousness of sensation due in part to amount of specific color-process and in part to amount of accompanying white-process.

Brodhun (who is green-blind, and skilled in such observations) found his mean error to be:

For comparison of red with red..... 3%
" " " blue with blue..... 3.8%
" " " red with blue..... 5.8%

(Since Brodhun and Ritter [red-blind] showed superior facility in these comparisons, it was at first supposed that that might be due to their color-blindness, but they have both since been somewhat surpassed by Frl. Elise Köttgen [*Helmholtz Festgruss*, p. 337]). It will be interesting to know how great is the mean error by the method of Prof. Rood. As regards the least difference perceptible by flickering between like colors (grays), it is very exactly the same as that found by König and Brodhun, for all colors and for white, at moderate intensities, but it would appear that, by the flicker-method, this is a difference which can be detected without trouble by untrained observers, and, what is most important, it would seem to be as readily observed between unlike colors as between like ones.

CHRISTINE LADD FRANKLIN.

BALTIMORE.

1. *A New Theory of Light Sensation.* CHRISTINE LADD FRANKLIN. Proceedings of the International Congress of Experimental Psychology, 103–108. London, 1892. Also reprinted in the Johns Hopkins University Circulars, XII. 108–110 (June 1893), and in Science, XXII. 18, 19 (July 14, 1893).
2. *Eine neue Theorie der Lichtempfindungen.* Same author. Zeitschrift für Psychologie, IV. 211–221. 1892.
3. *Theory of Color Sensation.* Same author. Science, XXII. 80, 81. 1893.
4. *Color Vision.* Same author. Science, XXII. 135. 1893.
5. *On Theories of Light Sensation.* Same author. Mind, New Series, II. 473–489. 1893.

Workers on the psycho-physiology of vision are indebted to Mrs. Franklin for the clear light in which she has set their problem. The second and fifth of her papers are the most important, the first being an abstract of matter more fully presented in the second, the third a reply to a criticism, and the fourth a notice of priority. A statement of the shortcomings of current theories naturally paves the way for the proposal of a new one. The theory of Helmholtz is at fault in that it disregards sensation in making white a mixture. While a combination of red, green, and blue lights does appear white, it is illegitimate to

infer that the white sensation is a compound of the red, green, and blue sensations. The theory is strained and artificial in its account of total color-blindness. Its explanation of complementary colored after-images with closed eyes by the 'self-light' of the retina is inadequate. Its explanation of simultaneous contrast has been proved to be wrong. Against Hering it is urged that his conception of reciprocal processes in the retina, both of which are attended by sensation (yellow, for example, being caused by a katabolic change and blue by an anabolic change in a single photochemical substance), is wholly unparalleled in the physiology of other sense-organs, sensation, so far as known, attending katabolism only; further, that the impossibility of color-intensity apart from color-whiteness (saturation) to which his theory leads is something not readily conceivable; that his explanation of simultaneous contrast is a mere translation of the facts into the terms of his theory without much demonstrative weight. Against this theory also the author brings the experimental objection that a gray resulting from complementary red and green and an exactly similar gray from complementary blue and yellow do not remain exactly matched when the total illumination is varied, as they must do on Hering's theory. The experiment was made, however, with the color-top and can hardly be regarded as conclusive. Hering himself denies the conclusiveness of similar experiments with spectral colors, unless made under special conditions, and asserts that when the experiment is properly made the matched grays remain matched at all intensities (*Pflüger's Archiv*, LIV. 299 ff., 1893). The theory of Donders is briefly criticised in the first paper above, and a criticism of Ebbinghaus' new theory is promised in the next number of *Mind*.

The author's theory, which is regarded, indeed, rather as an indication of the direction that such a theory should take than as a developed scheme, is in outline as follows. There are in the eye two sorts of photochemical substance: one, decomposed by all kinds of light, which gives, by the action of its decomposition-product on the nervous organs of the retina, the sensations of the black-gray-white series; the other, decomposed in particular ways by red, green, and blue lights, which gives these colors and their mixtures. When all three of these decompositions take place at once, the decomposition-product is the same as that resulting from the breaking up of the black-gray-white substance and accordingly gives the same sensation. Complementary pairs of colors give white because they cause all three of the decompositions at once; red and blue-green, for example, give white because red causes the partial decomposition corresponding to that color, and blue-green the decomposition corresponding to the

other two primary colors. The theory without great difficulty also accounts for the cases in which colors appear colorless,—e.g., at the extremes of intensity, when the retinal area is very small, when the eye is color-blind, etc.,—but on these the author lays little stress. The account that the theory gives of complementary colored after-images with closed eyes and of simultaneous contrast is regarded as following so unavoidably from the theory as to be a justification of it. When the eye is stimulated for a time with light of a given color (e.g., red) the corresponding decomposition takes place and the decomposition-product is used up in causing the red sensation. The molecules thus partially decomposed are in an extremely unstable condition, and in going to pieces set free the decomposition-products that give blue and green, whence the blue green after-image. In simultaneous contrast, where, for example, a red field induces a tinge of blue-green upon an adjacent gray field, the effect on the color-molecules is the same as that just mentioned, but in this case the mutilated molecules (or their decomposition-product) are carried out over the retinal area corresponding to the gray field by the rapid retinal circulation. The theory can easily be made to fit also, with the distribution of the rods and cones, the saturation differences of spectral colors, the retinal areas sensitive to different colors, and the unmixed appearance of yellow.

Difficulties and objections naturally suggest themselves with every new theory. What, for example, is this theory to do with black, especially with black in simultaneous contrast? And granting that the retinal circulation is rapid enough for the use made of it in explaining simultaneous contrasts (which seems doubtful to the reviewer), how is the reversal of colors which is found in the after-image of the contrasting fields to be accounted for? Finally, as the author herself observes, if Hillebrand's demonstration of Hering's theories of complementary colors stands, it is fatal to her theory, at least in its present form.

Some of the advantages of the theory can be inferred from what has already been said. It has three primary colors, an independent white, and need make no unusual physiological assumptions. Its chief advantage, perhaps, and one that it shares with the theory of Donders, is its use of partial decompositions in the photochemical substance.

EDMUND C. SANFORD.

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THE MUSCULAR SENSE.

Über den Muskelsinn bei Blinden. PAUL HOCHEISEN. Zeitschrift f. Psychol., v. 239–282. 1893.

The author shares the views of Goldscheider, under whose direction the experiments were made, as to the peripheral sources of the muscle-sense. Since the threshold of excursion for just perceptible active movements hardly differs from that for passive, we can use the threshold-values found for passive movements as measure for sensitiveness to active movements, hence, together with the extensive discriminative sensibility of the skin, as measure for the fineness of muscle-sense in blind persons.

Experiments were carried out on Goldscheider's apparatus for measuring movements. Three joints of each hand were investigated, both as to extension and as to contraction. An average of 100–150 trials was made for each determination, a total of more than 6500 experiments for determining movement-thresholds in the blind, with about 3000 more on seeing persons for comparison. Each determination of the skin-sensibility in different parts was the result of 50 trials with varying distance of the points. The conclusions arrived at were as follows:

It makes no difference for the fineness of sensation whether an individual was born blind or became so later. Blind persons who have cultivated the sense of touch show an objectively provable refinement of sensibility to passive movements, and thus of the muscle-sense; this refinement is, however, not very great and not present in all blind persons. The cause of the refinement is a psychical one, due to the fact that, through improvement of the attention and through practice in interpreting sensory signs, sensations of intensity usually indistinguishable are raised above the threshold. Children have a finer sensibility for movements than adults. Little difference can be found between right and left sides in case of movement-sensations. The discriminative sensibility of the skin is increased in slight degree, cannot always be clearly proved, and is due to practice.

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EXPERIMENTAL.

Minor Studies from the Psychological Laboratory of Clark University, I. Am. Jour. Psychology, v. 294–389. Apr. 1893.

1. THADDEUS L. BOLTON: *On the Discrimination of Groups of Rapid Clicks.*
2. MARY WHITON CALKINS: *Statistics of Dreams.*
3. F. B. DRESSLAR: *On the Pressure-sense of the Drum of the Ear and 'Facial Vision.'*
4. J. F. REIGART and E. C. SANFORD: *On Reaction-times when the Stimulus is applied to the Reacting-hand.*
5. JOHN A. BERGSTRÖM: *Experiments upon Physiological Memory by means of the Interference of Associations.*
6. JAMES H. LEUBA: *A New Instrument for Weber's Law; with Indications of a Law of Sense-memory.*
7. EDMUND C. SANFORD: *A New Pendulum Chronograph.*

Dr. Sanford explains in a brief preface that these studies were planned and carried out primarily as practice in research by his students. The studies are not for that reason either unimportant or untrustworthy, for the contributors have had good training, some of them are of independent strength, and all of them have enjoyed the intimate co-operation of Dr. Sanford. The title 'Minor Studies' is a very modest one, for it would be easy to find Doctors' Theses and other pretentious articles less significant and less thorough than some of these. For one thing, if for no other, these studies must receive the highest praise. One cannot find a line of verbal padding. Essential psychological explanations are put as concisely as are the statements of fact. This is a thing as excellent as it is rare.

1. The chief specific question considered in Mr. Bolton's paper is: "Can the presence of one click more or one click less be recognized when successive groups of different numbers of clicks are given, and under what conditions as to rate and number?"

The results with three subjects show that such differences can be recognized in a majority of the observations (57%–95%),—with two of these subjects in more than 75% of the observations,—provided that the rate be not more than about 133 per second for groups of 10 or more clicks, and not more than 153 per second for groups of 9 or less clicks. The fact is observed that the errors of judgment are fewer when the standard number of clicks is compared with a smaller group than when the standard is compared with a larger group. An attempt to measure the difference between the two cases showed that an increase

of two clicks is recognized more accurately than is a decrease of one click. For possible explanation of the difference in the two cases the following is proposed: "Our memory-image of the first actually increases in length between the first and second set of clicks. When the lesser group follows the standard, the second seems decidedly less than the first; but when the same or a greater number follows, the difference is not so great and more difficulty in discriminating is experienced." For explanation of the lengthening of the memory-image see, below, Mr. Leuba's 'Indications of a Law of Sense-memory.'

It is thought improbable "that any clicks are lost in the perception of a rapid group, at least up to 153 per second," since groups of 8 and 9 are distinguished at that rate. It is also thought improbable that the number of clicks in the group affects the impression of discreteness. The paper concludes with a brief statement by Dr. Sanford of the work of Dietze, Hall and Jastrow, and F. Schumann in the same field.

2. The 'Statistics of Dreams' is based upon "the accurate record of two people, from notes made by themselves during the night and supplemented by careful study and recollection on the following day." The observers were S., a man of thirty-two (taking records 46 nights, 170 dreams); and C., the writer, a woman of twenty-eight (taking records 55 nights, 205 dreams). A study of the results shows the following main results:

"Most of our dreams occur during the light morning sleep," but "the sleep of the middle of the night is in no sense a dreamless sleep." In a majority of the dreams, some connection with waking life could be traced, but "the influence of the time of the dream upon the degree to which it is associated with the waking experience" could not be accurately calculated. An attempted classification of the dreams according to their vividness showed that vivid dreams occur most frequently, but not always, in the morning. A small per cent of the dreams were classified as presentative,—visual, auditory, tactual, temperature, gustatory, and organic sensations appearing in the list, auditory sensations most frequently. The influence of actually present stimuli in occasioning such dreams is pointed out. The so-called 'representation dreams' were classified with respect to their representation elements. In this table visual elements greatly predominate. Word-dreams (spoken or heard) are very frequent, especially with the observer C., who reports dreams in five languages.

The connection between waking and dream life was studied by noting the presence and frequency in the dreams of perception, accurate and illusory memory, imagination, thought (contrary to Spitta), emotions of many sorts, and volition involving deliberation and de-

cision (contrary to a common opinion). There is noted further the presence in the dreams of persons, places, and other things from waking experience, together with the number and character of these. It is observed that many but not all (contrary to Délage) of these are of trivial importance. The connection of the dream with the waking life is put thus: "The dream will reproduce in general the persons, places, and events of recent sense-perception or of very vivid imagination—not the objects of ordinary imagination, of thought, of emotion, or of will, so far as these are not also perceived objects. Furthermore, thoughts, emotions, experiences, and personal relations that mean most to us are generally extremely complex and depend for their reproduction on the integrity of very many lines of association. When a number of these are put temporarily out of function by sleep, it is next to impossible to bring about these complicated mental states, though less complicated ones can be reproduced with tolerable completeness."

Differences between waking and dream life are pointed out,—as the isolation of the dreamer, partial or complete loss of personal identity, and various types of paramnesia, leading to various degrees of absurdity in the dream.

The writer questions the popular belief as to the swiftness of dreams, explaining the facts as resulting from a quickening of the memory-time, which makes what was dreamed in a long period seem to have been dreamed in a short one. This explanation does not, however, account for all such dreams, for in some cases the total time of the sleep is reported very short. The paper concludes with a brief consideration of prevision in dreams, with suggestions in explanation. The paper is by no means the statistical skeleton which its title and this statement of its contents might indicate, but is mature prose and good psychology. Even if it were not so, the collection of facts is very important. It is only to be wished that we had innumerable other collections as good.

3. Mr. Dresslar notes Prof. James' remark (*Princ. of Psychol.*, II. 140) that "the tympanic membrane is able to render sensible differences in pressure of the external atmosphere too slight to be felt as sound." Mr. Dresslar devised a simple apparatus for applying a measured pressure to the ear-drum and proved that "it is a mistake to ascribe to the tympanum a very great delicacy in the perception of pressure," and that accordingly Prof. James' explanation of the 'shut in' feeling sometimes called 'facial vision' is erroneous. Careful tests with three subjects verify the result that blindfolded subjects can distinguish in a majority of cases whether a frame brought near the side of

the face is open, solid, lattice, or wire. Temperature sensations were shown not to furnish the basis of judgment and the conclusion was reached that, after all, the basis of judgment is furnished by auditory sensations.

When a partial report of the foregoing result was made at the Philadelphia Meeting of the Am. Psychol. Assoc., Professor Ladd made the point that this is one of many cases of a kind of tact,—where we have fairly reliable knowledge without at all knowing through what means we obtain it.

4. The statement of Exner (Pflüger's Archiv, VII) that, contrary to his expectation, the reaction-time for electrical stimulation is longer by about 10σ when the stimulus is received by the reacting hand than when it is received by the other hand, was tested by careful repetitions of the experiment. The results show a slightly greater difference in the contrary direction.

5. Mr. Bergström took blank playing-cards, had abstract words or pictures of common objects printed upon these and so made up packs of eighty cards, ten kinds of cards to the pack. Having shuffled the cards so that two of a kind would not come together he directed his subject to sort the cards as rapidly as possible into ten piles, each pile to contain the eight cards of a given kind, and the order of the piles to be determined by a plan (see original) which prevented the subject from knowing the order in advance.

In the first set of experiments abstract words were used; two packs of eighty cards each, and alike in all respects except in the order of the cards, were placed before the subject. He was required to sort first the one and then the other pack, the order of the piles being in the two cases different and in both cases determined by the plan as aforesaid.

The results for five subjects show that the sorting of the first pack required from 122″ to 159″, while the sorting of the second pack required from 3.5″ to 18″ more time. That this increase of time was not due to fatigue is proved by the fact that when different words were used in the second pack, there was practically no increase of time.

To simplify the conditions, pictures of common objects were substituted for words. One pack was sorted and then after an interval of 3″, 15″, 30″, 60″, 120″, 240″, 480″, or 960″, a second pack was sorted in entirely different positions. Five subjects were tested, each at a given hour on successive (12 to 21) days. See the original for precautionary details. The results show that at each of the intervals the sorting of the second pack requires more time. The curve of

recovery as the interval becomes longer is determined. It was again proved that fatigue is not the cause of the retardation. Mr. Bergström believes the cause to be interference of associations and that his method is 'a laboratory method of studying habits.' The paper is short and the apparatus is simple and inexpensive; but the study deals with one of the most important problems of physiological psychology and does so with very considerable precision and success.

6. Mr. Leuba describes an apparatus devised by Dr. Sanford and modified by himself for testing Weber's law with lights, by the method of equal interval. For a detailed description of the apparatus (whose essential parts are a blackened tube and an episkotister) see the original. In using an apparatus copied after that of Dr. Sanford the reviewer found that the time during which the eye was directed into the dark tube made significant differences in the observation of faint lights. Whether this point was regarded in Mr. Leuba's work does not appear. The results obtained by him "do not show the uniformity of ratio required" by Weber's law, but "the deviations are not extremely great" and the series of magnitudes is much more nearly a geometrical series than an arithmetical. Mr. Leuba compares a series of experiments in which the standard lights had to be held in memory with a series in which the standards were visible all the time, and finds "a striking difference in the ratios at the lower end of the scale." He explains this outcome as follows: "The image of a recent sensation tends to recall, by association, the united residual of all the past sensations of the same kind, and in so doing passes over in some degree to this sub-conscious resultant impression." Hence "there seems to be a natural tendency in us to shift the sensation held in memory towards the middle of the scale of intensities." This the author thinks to indicate a 'law of sense-memory,' which falls in general line with results and theories in other fields of psychology.

7. Dr. Sanford's Pendulum Chronograph is simple and inexpensive, and under favorable treatment the gross error is reported not greater than 3σ or 4σ for short intervals and one part in fifty for times of a second or longer, "a degree of accuracy that is sufficient for all practice work in psychological time-measurements and for many kinds of research." For details of construction see the original.

W. L. BRYAN.

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Zur Physiologie u. Pathologie des Lesens. By Dr. GOLDSCHEIDER and R. F. MÜLLER. Ztschrft. f. klin. Medicin, XXIII. 1, 1893.

Recherches sur la Succession des Phénomènes Psychologiques. B. BOURDON. Rev. Philos., XVIII. 225–260. 1893.

Goldscheider and Müller have made an important series of experiments to determine whether, in reading, we 'spell out' letter by letter, or grasp several letters at once. By means of a slit in a rapidly rotating disk a number of symbols or letters could be exposed to view for a short but accurately measurable time. An exposure of .01 second (slightly above the lower limit of visibility) was used; more could be distinguished with this than with longer exposures. The symbols used were lines, arcs, squares, etc., very heavily printed. Any arrangement of three different, or four similar, symbols was generally reproduced correctly at once. More trials were required as this number was increased, except when the arrangement was symmetrical or suggested some well-known figure. Similarly, with numerals or meaningless combinations of letters, four were usually seen at first, five after a second trial, etc. *Words of five letters were generally read at once*, but with longer words the results varied considerably: mistakes were often made and persisted in, rendering repeated trials necessary. Three words of four letters were placed one above the other and exposed for .03 second; between eight and ten letters were recognized at the first trial. Any well-known phrase of three words could be read at once, but in this case the omission or misprinting of certain letters passed unnoticed. The writers draw a distinction between 'determining' and 'indifferent' letters of words. "The initial letter is almost always one of the determining letters." A vowel, when not forming a syllable by itself, is generally less important than a consonant, e.g., 'Kl ngb ld' was read as 'Klangbild,' but ' lan bild' was not completed. [The writers are in error when they speak of "recognizing" (erkennen) 'Charité' from 'Ch té,' 'Object' from ' bj t,' etc., for these might be completed in other ways; e.g., 'Subject'.]

The results show that "the time of recognition is dependent on the number of letters and the degree of familiarity" of the word. Except with unfamiliar words, we do not 'spell' in ordinary reading, but observe certain letters, or parts of letters, here and there, and fill in the rest by association—generally of the word-sound, but often of the word-picture. This renders improbable the existence of any 'memory-cells' for calling up memory-pictures by association with apperceived elements, for the memory-picture must be called up before apperception occurs, if it is to hasten and extend the apperception, as it does. The writers remark that in speech, unlike reading, there is always a

'spelling out of words.' But may not the muscular co-ordinations for speech proceed by syllables, or even phrases, as well as by single letters?

Bourdon uttered single words (or letters) at intervals of about four seconds; the subjects were to write down each time the first thing occurring to them. 1. A letter or simple sound (a, b, ch, gn, etc.) was given, with which a word was to be associated. Compared with the chance resemblance (calculated) of any word-pairs, the pairs thus obtained showed considerable phonetic and remarkable syllabic resemblance. 2. When a letter was required as 'answer,' alphabetic contiguity gave the association as often as phonetic resemblance. 3. If a color was required, the pair (letter and color-name) usually showed phonetic resemblance. The writer also claims association between 'open' vowels and bright colors, between 'dull' sounds and dark colors, etc. 4. A word was given, another word 'answered.' Here the meaning generally determined the association, the phonetic resemblance bearing about the random average, with important individual variations.

Distinguishing between names of qualities, objects and acts, the two latter were generally associated each with a word of its own class, the first about equally with qualities and objects. In general the two phenomena were co-ordinate, although with objects the second was often subordinate. They were rarely heterogeneous, but exhibited difference oftener than likeness (e.g., red and blue). Combining all these categories, the answers of some persons come under fewer headings than those of others, though greater in number; this indicates a more logical mind. To prove this, the writer analyses several passages typical of the literary and scientific styles.

HOWARD C. WARREN.

PRINCETON.

EPISTEMOLOGICAL.

Idealism and Epistemology. H. JONES. Mind, N. S. No. 7, 289–306, and No. 8, 457–472. July and Oct. 1893.

Metaphysic and Psychology. JOHN WATSON. Phil. Rev., vol. II. 513–528. Sept. 1893.

The Meaning of Truth and Error. DICKINSON S. MILLER. Phil. Rev., vol. II. 408–425. July 1893.

Old and New in Philosophic Method. HENRY CALDERWOOD. Phil. Rev., vol. II. 641–651. Nov. 1893.

The first of Prof. Jones' two papers may be called a defence of Neo-Hegelian doctrine, and the second an attack upon the attempt to obtain Reality by inference from subjective states. The charge made

against Hegelians and Neo-Hegelians, to wit, that they confound their thoughts of things with things, the Logic of Hegel with the system of the universe, is answered by the statement that their Epistemology is not at fault, for they have none, not recognizing two worlds to be brought together, but accepting ideas and things as manifestations of the one Reality and belonging to the same world. Yet Prof. Jones, while unwilling to call ideas 'existential realities,' admits that they have existence, and distinguishes between them as mere occurrences in consciousness and as having 'objective reference.' His position reminds one of Hamilton's distinction between the 'facts' of consciousness and the 'testimony' of the facts, and his opponent may still object that he appears to confound the idea and its 'reference' with the thing to which reference is made, assuming the latter given when the former is given. The second paper is largely an answer to Prof. Seth's article in the *Philosophical Review* (vol. I. pp. 504–517). The critical parts are more satisfactory than the constructive.

Prof. Watson's article is also written from the Neo-Hegelian point of view. It is a critique of Prof. Andrew Seth's position regarding the subject-matter of psychology, epistemology, and metaphysic, which allots to them respectively the self, the world, and God. Dr. Watson objects to regarding the world as known by inference from subjective states, and claims that one who does not start with 'reality' can never end with it. Mr. Seth's distinctions between psychology, epistemology, and metaphysic 'vanish away' when we bear in mind that "there is no consciousness of self apart from the consciousness of other selves and things, and no consciousness of the world apart from the consciousness of the single reality presupposed in both."

Mr. Miller's paper on the 'Meaning of Truth and Error' develops the consequences of the assumption that an idea can in no sense be said to reach out beyond itself and know an object beyond itself, whether this object be an external material thing or a past or future idea. It follows that truth and error must be found within the idea itself, must be gotten out of it by an analysis, for the mind is wholly shut up to its actual content at a single moment, the present. This doctrine, he maintains, is in no sense sceptical, permitting one to believe in an external world, a past and a future—it analyzes, but does not deny. The chief criticism to be made upon the article is that it does not distinguish between the psychological and the epistemological points of view, and even one who accepted its first assumption might desire to have some of its assertions pretty carefully restated.

Prof. Calderwood's paper is taken up with the problem of the significance for psychology and philosophy of the experimental and physio-

logical psychology. He points out with justice that there must always be ultimate reference to introspection whatever method be employed, and he also calls attention to the fact that the results so far obtained cover rather a narrow field, stating that they "do not constitute a psychology, or doctrine of the soul. At best, they supply a view of the soul's relations with its environment, more especially with its own organism, as the vehicle of sensibility and of motion. But mental action is not included. The further we advance into the knowledge of mind, the further we travel from the borderland on which the results are obtained." Dr. Calderwood concludes that experimental psychology can contribute little to a knowledge of the more complex mental phenomena, and that men must come and go by 'the old pathways of introspection' as before. One cannot object to the statement that ultimate recourse must always be had to introspection, in some sense of that term, but God forbid that we always follow the old pathways of introspection as they have been followed in the past. And one cannot object to Dr. Calderwood's statement of the present limitations of experimental research, but it is still too early to prophesy what may or may not lie outside its territory in the future. The tone of Dr. Calderwood's discussion and his estimate of the outlook for experimental work are evidently determined by his view of the soul and its environment. Were he an adherent of, say, the doctrine of a parallelism of cerebral states and mental states, it is probable that he would write differently on this question. G. S. F.

ETHICAL.

On Certain Psychological Aspects of Moral Training. J. ROYCE. Intern. Journal of Ethics, III. 413–436. July 1893.

Moral Deficiencies as determining Intellectual Functions. G. SIMMEL. Journal of Ethics, III. 490–507. July 1893.

The Knowledge of Good and Evil. J. ROYCE. Intern. Journal of Ethics. Oct. 1893.

The problem proposed by Dr. Royce is to discover the mental factors, or the psychological side, of the fact ethically called conscience; to show, so far as psychology is concerned, why we have a sense that conscience is immutable and authoritative, while, at the same time, it is historically fallible and variable. The judgments of conscience reduce themselves to two types: one advises sympathy, devotion to a will beyond one's own; the other, justice, reasonableness, the regulation of life according to a consistent plan. Morality is the complete union of these two principles, and comes into play when these motives

conflict either with opposing forces or with one another. Conscience, as thus defined, is our awareness of certain fundamental psychological tendencies — the tendency to imitate, leading to sympathy, and the tendency to form fixed habits, leading to regularity and consistency of conduct. The authoritative nature of conscience, its innate character, is due to the radical nature of these instincts. Its fallibility, its origin in experience, is due to conflict between these tendencies. Our imitativeness, our social suggestibility, act immediately, giving us generous impulses, but always tend to confuse our general plans. Our fixed habits, on the contrary, lend themselves to generality, but tend to become so fixed as to make us unmindful of the calls of sympathy.

The moral bearings of the discussion lie, of course, outside our scope. The psychological identification of imitation with immediacy of action, and of habits with reasonableness of action, seem to me, however, very questionable. Habit, as such, (apart from a need of changing it,) is, upon the whole, opposed to conscious reflection: and one could make out a very fair case for the hypothesis that imitation is one form of the law of habit, instead of a principle opposed to it. Dr. Royce, to be sure, makes much of the element of conflict, but only as affecting the ethical value, not as having an intrinsic psychological significance.

Simmel's extremely acute essay falls within our range so far only as it deals with the psychological and anthropological relation existing between knowledge and intellectual acuteness on one side, and activity denominated moral on the other. It is noteworthy for maintaining the anti-Socratic paradox that immorality is important to intellectual development in certain directions. It is a fundamental principle of modern psychology that an idea of an act is the first inclination to its execution; that, indeed, there is an organic psychophysical connection between a conception of an act and its performance. This granted, it follows that complete recognition of the act can be had only by following the idea to its consummation—that the act *is* the idea consummated. Hence immoral acts are the condition of our comprehension of immorality; the reproduction of the evil passion the only way to know it. This general principle is reinforced by considerations from criminal anthropology—the immoral man swims against the stream, and, hence, requires more strength, acuteness, etc., to succeed. Through lying, the mind grows wary, comprehensive, delicate and strong, acquires a good memory, quickness of invention, power of imagination, etc. A third psychological connection is found in the relation of the emotions to knowledge. Morality requires interest, sympathy with subject-matter; science requires indifference, approach-

ing hard-heartedness—e.g., vivisection. Development of æsthetic power, regard for the picturesque, etc., requires also quiescence of altruistic emotions.

Dr. Royce's second article (considering the same problem as the foregoing, and, in part, a reply to it) attempts to reduce the apparent conflict between the demands of morality and those of intellectual progress to one special case of a more general law, psychological and even biological in character. This law is that every organic process is the combination in harmony of opposing tendencies: living tissue involves, as part of its own activity, phenomena which by themselves would mean death; every voluntary movement, action on the part of antagonist muscles; every nervous stimulation a corresponding inhibition; every virtuous act a known tendency to evil. In each case, the organic activity involves the reduction of an opposing tendency to a contributing factor in the activity itself. Applying this to the paradox of Simmel, it follows (1) that knowledge of evil does not require the actual evil-doing, but simply the presence of an evil tendency; and (2) that there is deep insight into the nature of the evil deed only so far as it is transcended; this being, apparently, a special case of the general law that we do not truly know any activity as long as we remain in it, but only when, by getting beyond it, we are able to turn back on it as an 'object' of reflection.* The same principle is involved in the fact that the attainment of virtue involves a constant approach to a condition where evil motives have no force and are ignored. Here the question is as to the relation of habit and consciousness. We are completely conscious only when the function concerned is learning, only when it is novel; mastered, or become habitual, it passes into unconsciousness. We cannot affirm from this, however, that consciousness is aiming at its own absolute extinction, for this unconscious function is the instrument for mastering wider situations and thus subserves a wider consciousness.

Dr. Royce would have made his case still stronger, psychologically, it seems to me, if he had not admitted that the evil tendency is evil *per se*. It is difficult to see how any organic process can be bad in and of itself. It is *in* entering into a larger activity, of which, therefore, it must become an inhibited factor, that it becomes bad as it would be in itself. Instead, then, of saying with Simmel that only the bad man can know evil, it is a psychological necessity to say that only the good man can know it—know it, that is, as evil. J. D.

*I say apparently, because, while this is implied, I do not quite know whether Dr. Royce expressly means it.—J. D.

NEW BOOKS.

Grundzüge der physiologischen Psychologie. WILHELM WUNDT. Vierte umgearbeitete Auflage. Engelmann, Leipzig, 1893. Pp. xv, 600, and xii, 684. M. 12 and 10. (The third edition is here enlarged by 181 pages.)

Grundriss der Psychologie auf experimenteller Grundlage dargestellt. OSWALD KÜLPE. Engelmann, Leipzig, 1893. M. 9.

Psychologie des Erkennens vom empirischen Standpunkte. GOSWIN K. UPHUES. Erster Band. Engelmann, Leipzig, 1893. M. 6.

L'Evolution Intellectuelle et Morale de l'Enfant. G. COMPAYRÉ. Hachette, Paris; 1893. Pp. xxiv, 371.

The Psychology of Childhood. F. TRACY. Heath, Boston, 1893. Pp. 94.

Appearance and Reality. A Metaphysical Essay. F. H. BRADLEY (Library of Philosophy.) Swan Sonnenschein, London; Macmillan & Co., New York; 1893. Pp. xxiv, 558.

NOTES.

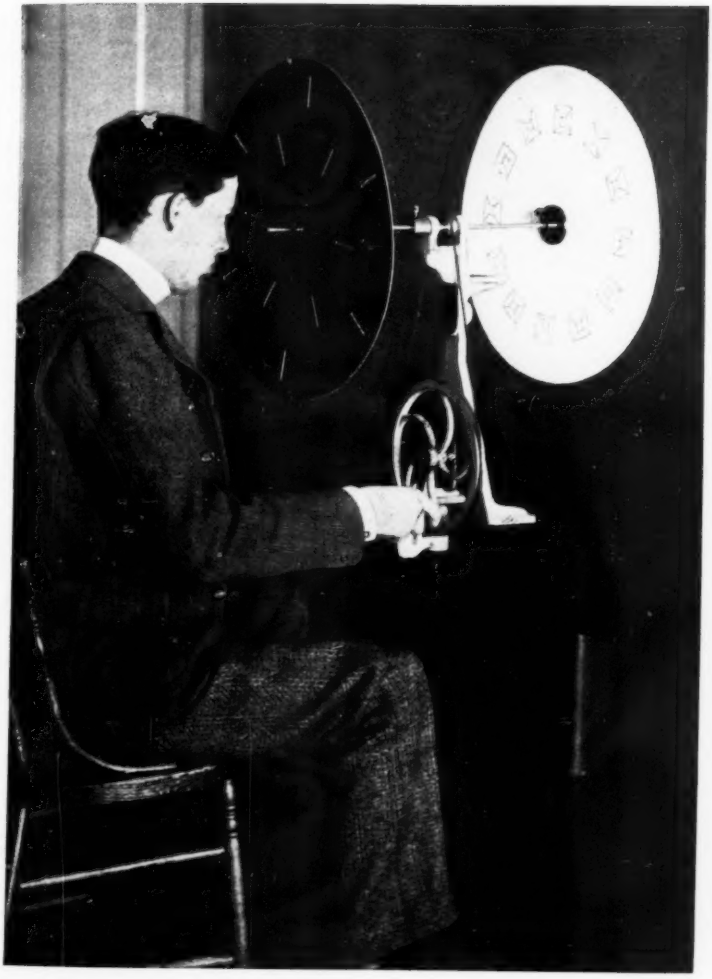
Prof. STUMPF has accepted a call to the Berlin chair of Philosophy, which will be vacated by Prof. Zeller on attaining his eightieth birthday.

According to *Nature*, Drs. G. DWELSHAUVERS and P. STROOBANT will have charge of a laboratory of 'Psychological Physics' in the University of Brussels; and Dr. W. H. RIVERS, of St. John's College, will conduct practical work in the Psycho-physical laboratory at Cambridge.

Prof. ALEXANDER, who takes the chair in Owens College, Manchester, vacated by the remova lof Prof. ADAMSON to Aberdeen, will offer opportunity for work in Experimental Psychology.

A. KIRSCHMANN, Ph.D., and F. TRACY, Ph.D., have been appointed Lecturers in Philosophy in the University of Toronto. Dr. Kirschmann will have charge of the Psychology, and will direct the laboratory.

JAMES R. ANGELL, A.M., has been appointed Instructor in Philosophy in the University of Minnesota, and offers laboratory courses in Experimental Psychology.

HOWARD C. WARREN, A.M., has been appointed Instructor in Experimental Psychology in Princeton College.

LIVINGSTON FARRAND, A.M., M.D., has been appointed Instructor in Physiological Psychology in Columbia College.